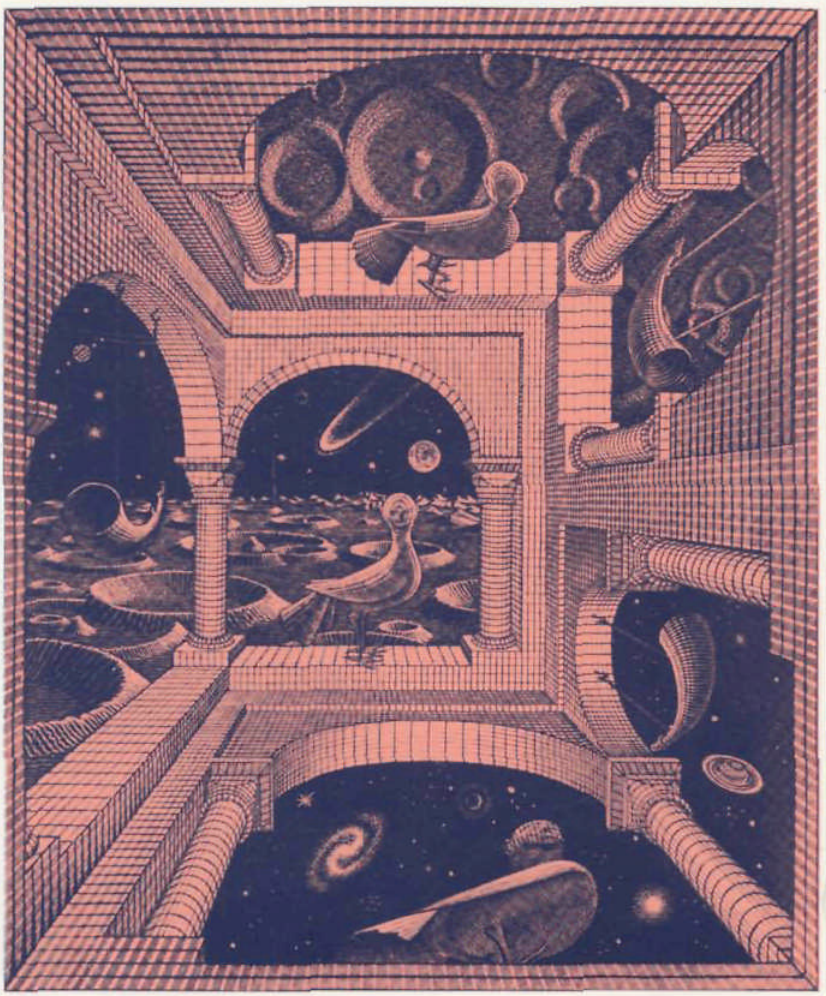
the Skeptical Inquirer

THE ZETETIC

Science and Pseudoscience



Superstition Old and New / Psychic Archaeology
Voice Stress Analysis / Mars Effect Follow-up

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the Skeptical Inquirer
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News and Comment

Science and pseudoscience: AAAS explores the issues

The annual meetings of the American Association for the Advancement of Science are the only regular large get-togethers in the United States where scientists from diverse fields discuss scientific issues of general public interest and importance. Several past AAAS meetings have had memorable sessions, notably the UFO symposium in 1969 and the Velikovsky symposium in 1974, in which scientists attempted to deal with the vexing issues of claims from the outer fringes of science. This year's AAAS meeting in San Francisco in January featured a perhaps less dramatic but nevertheless fascinating session on science and pseudoscience. This time the focus was not on one particular fringe-science claim but on some of the more general issues and problems that surround the subjects of pseudoscience and borderline science.

The intent, in the view of symposium organizer Rolf M. Sinclair, a National Science Foundation physicist, was to explore what science is and is not and to consider some distinctions between good science and bad science and between science and pseudoscience.

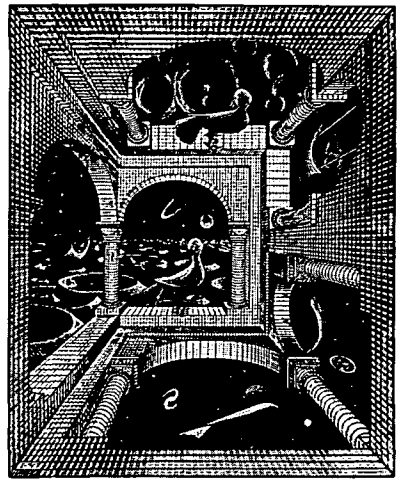
Sinclair himself set the tone with a low-key opening talk in which he informally put forth some of his thoughts on the subject. He emphasized that there are really no boundaries to what science can study if the work is done properly. Science, which is a unique process of discovery, is often incorrectly perceived to be a monolith. Yet it is highly diverse and "incredibly competitive." Scientists, he agreed, are hard on what they consider nonsense, and most of the time they apply that label it is with good reason. Yet the job of science is "to winnow out unprofitable viewpoints" rather than to suppress them. Sinclair pointed out that if science wanted to suppress unsettling new evidence, as pseudo-scientists often accuse it of doing, it probably would have suppressed such revelations as the evidence of cosmic blackbody radiation, which led to the downfall of the steady-state universe and ushered in the less aesthetic and more troublesome expanding universe cosmology. Yet the idea was readily accepted, whatever the inconveniences it caused.

It is this rapidly changing character of science, this ability to quickly accept new ideas when the evidence supports them, that is one of the key distinctions between science and pseudoscience. In fact, Sinclair emphasized that virtually all of what

science deals with—at least that aspect of science that involves the cutting edge of discovery—was new in the past 10 to 15 years. With pseudoscience, viewpoints are clutched at doggedly for their own sake, with astrology being a prime example. "Astrology froze about 2,000 years ago and simply hasn't changed much since," Sinclair pointed out. "That unchanging character is what allows me to say astrology is a pseudoscience." This is related to another feature of bad science, or pseudoscience—the "flagrant disregard" of the existing body of scientific knowledge on the subject.

It is always difficult, Sinclair acknowledged, for a scientist to decide whether it's worth his time to deal with a perhaps well-meaning amateur pleading to be heard. If a scientist "gets glassy-eyed" over the latest claims of the angle trisectors, the circle squarers, the perpetual motionists, and the like, it's "because he's heard it a hundred times before." And some ideas, while they theoretically can't be ruled out, are void of content. An example might be the idea that the world is 6,000 years old and that all the geological and fossil evidence to the contrary has been put here to test our faith, or that the world is five minutes old and we have all been created with memory patterns making us think otherwise. "I can't disprove that," says Sinclair, "but it's an empty viewpoint and I don't intend to deal with it." Occam's Razor, the rule that the simplest explanation consistent with the evidence holds priority, is a powerful tool for cutting through such contentions.

Polls showing a high incidence of public belief in such subjects as UFOs are hardly surprising, given our bombardment with the tabloid headlines at supermarket checkout counters, notes Sinclair. Yet science isn't done by polls. You don't take a poll



Escher Foundation

"about what theory of particle physics" is correct. "You may argue the points, but you don't vote on them."

In conclusion, Sinclair contrasted the revolutionary effect that molecular physics has had on biology in the past quarter-century to the absence of any effect of pseudoscientific ideas. "Some very well plowed fields have been with us now for quite a long time but have yet to have any effect on the body of science or on human action." And the value of a scientific idea, he noted, is measured to a large degree by its effect.

* * * * *

Sociologist Rodney Stark, of the University of Washington, presented results of research he and his colleague William Sims Bainbridge conducted on the geographic distribution of cult groups and the strength of organized religion. It turns out that cults are strongest where organized religion is weakest, notably in the western United States and specifically in the Pacific Coast states, from Mexico to Alaska. A report by Bainbridge and Stark outlining much of this work appears in this issue. Edwin C. Krupp, director of

the Griffith Observatory, gave an entertaining presentation on fringe-science conceptions about the past, such as lost continents, ancient astronauts, and pyramidology, "notions that represent nostalgia for a golden age."

* * * * *

The symposium was listed in the "History and Philosophy of Science" section on the program of the AAAS meeting, and the talk by psychologist Ray Hyman, of the University of Oregon, was properly historical: It drew lessons for the present from the past. Hyman's theme was the scientific community's handling and mishandling of pathological science, a term used by the late Nobel laureate Irving Langmuir for "the science of things that are not so," broadened by Hyman to also include cases in which scientists have wrongly insisted something wasn't so.

Specifically, pathological science refers to cases in which certain scientists have become strongly committed to a belief that is considered bizarre or discredited by most parts of the scientific community. Prominent examples from the past include Alfred Russel Wallace's involvement in spiritualism and Sir William Crooke's belief in the existence of a psychic force. In both cases, highly respected scientists simultaneously carried on their regular scientific work with success and acclaim while their "deviant" scientific work was either ridiculed or ignored and eventually banished from further mention in scientific literature, textbooks, and educational institutions.

Hyman believes this typical reaction of the scientific community to pathological science is bad for science and for the public. One of the dangers is that it forces the offending scientist into dealing only with committed believers who share his views. This "enforced compartmentalization" isolates him and his claims from orthodox science and removes any further incentive to correct or remove the loopholes in his position. "This further entrenches him in his belief that his initial claims were correct."

Too often, says Hyman, the criticisms of his claims are inept, based on "misrepresentations, irrelevancies, character assassination, plausibility arguments," and the like. This strengthens the scientist's belief that he is being treated unfairly and drives him further into the closet.

Discrediting the "deviant" scientists causes episodes of pathological science to be eliminated from the science-history books and textbooks, and that is a serious mistake. "By banishing the failures of otherwise accomplished scientists," says Hyman, "we prevent the learning of any lessons from them. Future scientists not only do not learn why and how such failures occurred, they do not even learn that they did occur. . . . If we keep future generations of scientists ignorant of the follies of some of their most accomplished ancestors, how can we hope to prevent repetitions of these very same follies?" The whole process, warns Hyman, reinforces what has been called "the illusion of personal invulnerability," the idea that "it cannot happen to me."

"In fact," says Hyman, "it not only can happen today, but it appears to be happening with increasing frequency." He points to the recent examples of prominent scientists' endorsements of "the conjuring antics" of Uri Geller and other scientists' claims that certain psychics can indeed project thoughts onto camera film.

"I strongly urge the scientific community to bring the pathologies out

of the closet and to work openly toward developing a more appropriate and rational response," says Hyman. "The fact that such pathologies have occurred and continue to occur should be taken as a sign that all is not well."

Furthermore, focusing the debate on the claims and evidence, rather than on the "heretical" scientist personally, will force him to "respond with further replication, refinements, and controls. At the same time, it forces the critic to show not only that the proponent is wrong but in what ways he went wrong. This could lead to a better understanding of how trained scientists can be trapped into defending false systems."

* * * * *

The real drama in the symposium came with the presentation by James Randi. Randi put his skills as a magician to work to demonstrate to the audience how easy it is to stage "miraculous" events of a type often referred to as evidence of the paranormal. He produced a "psychic photograph" on stage using an unexposed sheet of Polaroid film and passed the exposed print (showing a woman and a child) around the audience. While the audience and cameras crowded around for a closer look, he carried out "psychic surgery" on a volunteer subject, relieving the "patient" of a vast quantity of "blood and guts" from his unincised stomach. And Randi amazed a sizable proportion of the audience by successfully predicting which one word a member of the audience would select from a newspaper clipping. After the volunteer announced the selected word ("wonder"), Randi instructed him to look beneath his chair. He found fastened to his seat a sealed envelope bearing a letter notarized eight days earlier stating that the word that would be selected would be "wonder."

The demonstrations were entertaining, but Randi's point was serious. "I assure you," he said, "that I do not take the subject lightly, nor do I share the attitude of many of my colleagues in the conjuring profession that [deception by magicians posing as 'psychics'] can do no harm."

Randi recalled his more than three decades of investigating claims of paranormal abilities and occurrences. "The vast majority have been, as expected, mistaken interpretations of perfectly ordinary events, which, flavored by the beliefs of the times, tend to take on an extraordinary character and, fed by hyperbole and subtle changes in the basic evidence, can easily become fixtures in the mythology of the occult."

He warned scientists not to assume they have special abilities to spot cheating by paranormal claimants. This attitude has often led to a scientist endorsing someone's psychic abilities, only to have to retract the endorsement later amid considerable embarrassment.

Randi concluded by referring to the call by physicist John A. Wheeler at the AAAS meeting a year ago to discontinue the parapsychologists' affiliation with the AAAS. He said he applauded Wheeler's "forthright attack on the flummery of the matter," but suggested instead that the AAAS keep the parapsychologists in their present affiliation "but demand . . . to see results produced under proper experimental design, conduct, and evaluation." He feared that to throw them out would be "to make martyrs of them" and produce accusations that the AAAS "is just too fuddy-duddy" for their discoveries. "No, keep them close at hand, if you can, and demand results." The CSICOP, of which Randi is a founding

Fellow, has also gone on record against any movement to oust the parapsychologists.

* * * * *

The prepared talks at the symposium were followed by more than an hour of discussions and lively give and take with the audience. Everyone got to have their say, and the participants, organizers, and at least most of the audience seemed to consider the symposium a great success.

A two-cassette set of audio tapes of the session (tapes 17A and 17B) is available by sending a check for $17 to the official recorder of AAAS sessions, the Mobiltape Company, Inc., 1741 Gardena Ave., Glendale, Calif. 91204. All speakers' presentations are on the cassette set except, for some reason, Randi's. Its absence is undoubtedly due to a fit of telekinetic erasure by one of his antagonists.

—Kendrick Frazier

ESP and statistics: The case for skepticism

Persi Diaconis has a rare combination of expertise. He is a professional mathematician and statistician with a knowledge and interest in the conduct of ESP experiments, and he has been a professional conjuror (10 years on the road as a magician), an experience that has given him considerable skill at detecting sleight of hand and other manipulations of our minds and perceptions.

Diaconis, assistant professor of statistics at Stanford University, brought both these attributes to the AAAS meeting in San Francisco earlier this year, where, at a well-attended session on the frontiers of the natural sciences, he gave a lively invited presentation on "ESP and Statistics."

Attempts to demonstrate the existence of extrasensory perception are, Diaconis notes, essentially statistical in nature. At least that's true of the three main "kinds" of supposed ESP: telepathy, clairvoyance, and precognition. In virtually all laboratory experiments in these areas, claims of success rely upon small differences between actual outcomes of repeated experimental runs (such as the number of cards guessed correctly in multiple trials) and calculations of the expected outcome if only chance is at work. Often, says Diaconis, the mathematical assumptions made in calculating "chance" are not reasonable.

And just as often chance isn't the only force operating on the outcome of the results. Simple chance is the legitimate basis for comparison only when there is no feedback of any kind given the guessing subject during the experiment. Diaconis's experiences as a witness to many ESP experiments convince him that some degree of feedback frequently is in operation during the experimental runs but not mentioned in the formal written reports of the experiments. Sometimes the feedback is subtle and the experimenters are unaware of it, as when a subject might be able to tell from the "sender's" facial expressions whether he guessed right or not.

In any event, unreported feedback completely negates any attempts at statistical analysis of an experiment. Combining the feedback statistics problem with his observations of loosely controlled conditions in many of the experiments he's seen, Diaconis finds himself skeptical of the successful results claimed in the parapsychology literature.

The statistics of ESP experiments are complicated enough when all feedback is acknowledged. Take the

simple case of a well-shuffled deck of 52 playing cards, where there's a "sending" subject concentrating on each card in turn and a "receiving" subject attempting to guess the correct suit and number.

If no information is given the receiving subject at any time, the odds of his guessing any given card right is 1 in 52; and since there are 52 cards, there would be 1 correct guess on the average (with a wide distribution of the number of correct guesses depending upon guessing strategy).

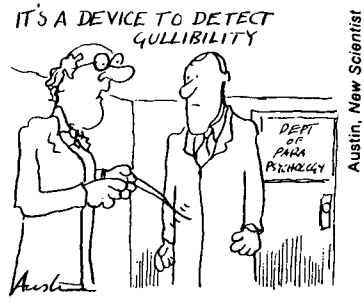
If there is complete feedback, the odds jump enormously. In this case, after each guess the subject is shown the card. He therefore no longer will guess that card, guessing instead any other card in the deck. This strategy yields an expected number of correct guesses of about 4.5, much-improved odds.

More difficult is the in-between case. Here the subject is told only whether each guess is right or wrong. The optimal strategy with playing cards is then to guess one particular card each time (say the ace of spades) until correct, then switch to another card. This way the expected number of correct guesses in a 52-card deck is 1.7.

The same analysis can be used with the Zener cards typically used in ESP experiments. A 25-card Zener deck consists of five different symbols repeated five times. The number of correct guesses expected by chance, using the optimal strategy in one run through the Zener deck, work out this way:

No feedback	5.00
Total feedback	8.65
Yes-no feedback	6.63

The third figure was difficult to determine, as there is no single optimal strategy. In fact, says Diaconis, it required about 12 hours of computer



time at Bell Labs to calculate.

Many ESP experiments are more complicated than these examples. The point is that the odds go up considerably from what is often referred to as "chance" if there is any feedback to the subject. As Diaconis says, "Feedback, even trivial amounts, makes a difference."

How does all this relate to what actually goes on in ESP experiments? Diaconis points out that the 6.63 figure "is in the ballpark for what successful studies get right."

The numbers are suggestive. The implication is that in some reported experiments claiming to show evidence of ESP, there may be nothing more important happening than feedback causing "above chance" results.

Does it really happen? "I think informal yes-no feedback does go on," says Diaconis. "So the 6.63 figure is a baseline. My belief is that in many less tightly controlled experiments you have that feedback operating."

Diaconis says that on the surface the published reports of such research "appear to be quite impressive. There are about 700 to 1,000 experiments reported in refereed journals, and about three-fourths are positive." In the written reports, he says, all is neat and tidy. Everything appears to be in order. "But it's not enough to look at the reported research. You have to know what goes on in the experiments," and

what goes on, he says, is not always fully reported in the journal articles.

As he did in his revealing article on statistical problems in ESP research in *Science* two years ago (vol. 201, July 14, 1978, pp. 131-137), Diaconis relates the story of his experience with a supposed psychic known in the literature as B.D. Diaconis was invited as a magician to observe B.D. in some demonstrations at Harvard in 1972.

All was not as it seemed. Diaconis's close observations showed that B.D. was a skilled opportunist, never specifying in advance the result to be considered surprising (thus giving himself great leeway) and, like a magician doing card tricks, always exploiting every coincidence to his advantage and rationalizing the misses. In a variety of ways familiar to magicians he was vastly increasing his odds of success over what they seem to be to the unwary observer.

Also, Diaconis says that on several occasions he observed B.D. doing sleight of hand, using some standard magician's tricks. Later, in a hallway, a colleague stopped Diaconis and said, "I understand you think B.D. used sleight of hand." Diaconis recalls replying, "I don't *think* he used sleight of hand. I *know* he used sleight of hand. There *is* a difference."

Yet experiments with the same B.D. were reported the next year in a parapsychology journal and were considered to be exciting evidence of ESP.

As for the infamous Uri Geller, Diaconis told the AAAS audience, "Every time I saw Geller he used sleight of hand." Then he added, "But I only saw him [here Diaconis paused] 20 times. Maybe he never cheated any of the other times!"

Parapsychologists cry foul over Diaconis's references to now-dismissed psychics. And they say the cases Diaconis refers to are atypical and not representative of most laboratory ESP research. Diaconis disagrees. "My story is typical of all my experience with ESP experiments." In answer to a question from the audience about the research in these areas at SRI International (formerly the Stanford Research Institute), Diaconis said that the SRI research is "some of the most publicized and least valuable. I witnessed some of the experiments. They were unbelievably sloppy. I really observed sloppiness that was beyond good taste."

In his talk, as in his *Science* article and in later defenses of criticisms of it (*Science*, letters, Dec. 14, 1978; *Zetetic Scholar*, no. 5, 1979), Diaconis repeatedly emphasized that confusing and erratic test conditions are typical of every test of an alleged paranormal phenomenon he has witnessed. He believes strongly that the published reports of experiments are not adequate to properly evaluate details of the conditions that were in effect.

There has been resentment among parapsychologists that general scientific journals, notably *Science*, do not publish ESP experiments. Diaconis says he would not support their doing so until the problems he has referred to are corrected and until expert magicians are allowed to witness experimental procedures.

As for now, Diaconis, told the AAAS in conclusion, "In my experience, there is no convincing evidence that a real, repeated phenomenon has been documented."

—*Kendrick Frazier*

The Persian Gulf UFO: Fiery drama comes to an end

Yet another UFO report from the *National Enquirer* has demonstrated the unreliability of UFO eyewitnesses

and their unwillingness to consider prosaic explanations in the excitement of having seen a "true UFO." And if further demonstrations were needed, the case also shows the ease in which extremely poor UFOs—i.e., those solved with less than five minutes worth of investigation—can slip into the UFO data base, there to serve as traps for the unwary.

This case occurred on the evening of August 24, 1979, over the oil-rich Persian Gulf. "Flaming 6,000 m.p.h. UFO Explodes in Midair" was the tabloid's headline the following November 6, when the case was finally reported. The lead paragraph was especially provocative: "In one of the most widely reported UFO sightings ever, hundreds of witnesses—including veteran airline pilots, air traffic controllers, and government officials—watched in stunned disbelief as a metallic UFO plunged toward earth in the Middle East."

The first to see the object was a British Airways pilot, who said it was flying at an incredible speed between him and a Japan Airlines craft. Kuwait Airways flight 370 then reported "a bright metallic object." Additional reports came from Scandinavian and Swiss airliners.

Ground observers saw it as well. According to Shoori Ghawas, an air traffic controller in Bahrain, "It was shiny and metallic, with red flames shooting from the rear. Then suddenly the center of it turned a bright, glowing red and there was an explosion and it disappeared from sight." Ghawas was particularly baffled by a feature that was reported from other airports as well: the object did not appear on radar!

George Williams, editor of the English-language *Gulf Daily News*, also was a witness. "We were all just standing there hypnotized as the UFO zig-zagged across the sky," he told an

Trajectory of Cosmos-1123 booster

interviewer. "It seemed to stop and hover over a brightly lit mosque for a few seconds . . . I'm convinced I saw a spacecraft from another world." Another witness was a veteran off-duty pilot named Norman Vacher. "I was stunned," he recalled. "I have never seen anything like it. I quite honestly could not believe what I was seeing."

Official government probes were launched in Bahrain and Qatar, and both groups concluded that it had been a genuine UFO. The leader of the Qatar group was quoted as saying, "My final conclusion was that the object was definitely a UFO under its own power which eventually exploded." The Bahrain group calculated that the object was going in excess of 6,000 miles an hour. Further, the group wrote:

"This is the most positive sighting by reliable witnesses ever in history . . . This was definitely not refracted or reflected light, nor a meteor or satellite, or any known flying craft made by man."

As the months passed, no new data came out; but the tabloid's story was picked up by pulp UFO magazines and by various UFO club newsletters. Its appearance in the next generation of UFO books is only a matter of time.

Nobody seemed to want to check up on the case, however, To experienced UFO investigators not out for sensationalism or exploitation, the descriptions sounded very much like a re-entering satellite or a meteor—despite the disclaimer of the "official" probes. The odds are that meteors leave no records behind, but falling satellites do.

This one did. A telephone inquiry to the North American Air Defense Command hit paydirt. "We have a match," public affairs officer Sgt. Mike Bergman reported a day after my phone call. "The rocket booster of Kosmos-1123 was coming down in that area at that time along a north-to-south trajectory." Using computer tracking data supplied by NORAD, it was possible to plot the path of the dying satellite; it zipped right across Bahrain and Qatar at an altitude of about eighty miles and a speed of 18,000 miles per hour (ironically, the *National Enquirer's* speed estimate had been *low*!).

The object had helped carry the Kosmos-1123 spy satellite into orbit three days earlier. Now the 20-foot-long cylinder, its mission accomplished, was falling back to a fiery death over the western Indian Ocean.

The incorrect perceptions of the eyewitnesses are not at all uncommon in such cases. Pilots can almost be counted on to estimate fireball meteors and burning satellites as being "very close" when they are scores of miles away. Balls of fire will be described as "metallic" when all they are, are balls of fire. People on the ground will misjudge, misremember, and misreport the motion of lights in the sky, and will attribute motivations to them (such as the reconnaissance of the brightly lit mosque) without any basis in fact. And another factor was typical: all efforts to contact the "official organizations" and the newspapers that carried the UFO story were fruitless. No retractions were ever printed.

In this case, we were lucky enough to be able to determine the actual cause of the "UFO," but it will not help since most of the public won't hear about it. It *will* help, however, in judging the reliability of similar reports from witnesses with similarly impressive credentials, even though we are unlikely to stumble across recorded data on the actual prosaic stimuli that lead to such reports. Such cases as this one go a long way toward explaining why a few percent of UFO reports will always remain "unsolved" even though no alien-extraordinary cause may be involved.

—James Oberg

James Oberg is a computer systems specialist at the Johnson Space Center in Houston.

Psychic archaeology in Egypt: Quest takes a dunk

"Cleopatra's palace found with the aid of American psychics." Such was the message disseminated internationally last January in news reports from a conference on underwater archaeology in Albuquerque, N.M., and in simultaneous press statements from Cairo.

The articles related how Stephen Schwartz and a Los Angeles based underwater archaeology research group he headed had found the ruins of Cleopatra's palace and other archaeological treasures below the waters of a harbor at Alexandria, Egypt. As part of the search, maps and questionnaires were sent to 11 psychics. Each psychic was asked to describe the structures as they were before their destruction. In addition, the psychics were asked to identify the location of each ruin on a map of the harbor. After getting the answers, Schwartz said, the areas marked by the psychics were overlaid on another harbor map. The team then looked for areas of consensus. Schwartz was quoted as saying that no psychic has "a better than 50 percent accuracy level," but he added, "with consensus, we obtain very reliable responses."

Using the psychics' locations, Schwartz said the team then surveyed the harbors using sonar equipment and divers. They said they found some remains of the palaces of Cleopatra and Marc Antony, including a pillar, and some materials related to the Lighthouse of Pharos, one of the seven classical wonders of the world.

A slightly different perspective on these discoveries emerged some ten days later. An Associated Press reporter talked to two prominent Egyptian archaeologists in Cairo. They disputed the claims.

"It's nonsense," Shehatta Adam, head of Egypt's Antiquities Department, was quoted as saying. "They are just seeking publicity."

Mustafa El Abazy, chairman of the Classical Civilization Department at Alexandria University, said there was no proof the relics belonged to the palace of Cleopatra, or to any other palace for that matter. "The pillars could have been part of support structures for the ancient harbor facilities."

"There is no archaeological evidence to indicate that this is Cleopatra's palace," said Adam. "We know that in the time of the Pharaohs there was a harbor in this location and our divers have found pillars and houses underwater. But what is there to connect with Cleopatra? It is nonsense."

Adam said the Antiquities Department had refused the group's request for a photo permit to explore the eastern harbor and had taken steps to halt the project in the fall of 1979 because "it was not a serious group." He said such exploration privileges are granted only to authentic scientific organizations.

—*K.F.*

NBC disclaimer on "psychics"

There's good news and bad news from the National Broadcasting Company.

The bad news (for those who lament the popularity of proclaimed "psychics") is that WRC-Radio, the NBC station in Washington, D.C., began an hour-long call-in program five days a week with a claimed "psychic" named Sallie Rigler in January, 1980. The program has resulted in as many as 14,000 attempted calls a day into WRC, according to operations director Gordon Peil.

The good news (although it's clutching at straws) is that NBC's legal department in New York has required WRC to read this disclaimer at the beginning and at the end of each program:

> WRC is presenting this feature solely for entertainment purposes and does not intend, by the presentation of the

material, to attest to or affirm its accuracy or integrity, or foster a belief in the subject matter.

Peil told the SKEPTICAL INQUIRER that WRC had asked NBC Legal what it needed to do to be able to present the call-in program. The mandatory disclaimer was its answer. He believes it pertains only to WRC and this particular program. He confirmed that it is read on the air at the beginning and at the end of each hour with Rigler.

Told of concerns about "psychics" misleadingly claiming to have the ability to foretell the future, Peil said, "We present this only as entertainment."

—*K. F.*

"Factual" UFO documentary leaves out some facts

The Center for UFO Studies (CUFOS) has recently served as the scientific consultant for a new phonograph documentary produced by a Chicago group called Investigative Research Associates. This two-record set is titled "Factual Eyewitness Testimony of UFO Encounters," and it is very favorably reviewed in CUFOS's allegedly monthly publication, *International UFO Reporter*. Since CUFOS purports to be the most genuinely scientific UFO investigative body in America, and since its director, J. Allen Hynek, the best-known UFO proponent in America, speaks "for the record" in seven different places, one should expect this recorded documentary to be the best that the pro-UFO forces have to offer. "Investigative Research Associates has been proud to have Dr. Hynek direct this project, to prepare the first completely factual documentary on UFOs," the record states.

What does this "completely factual" documentary on UFOs offer the listener? Its first major case is the Jimmy Carter UFO sighting, presented as if it were a genuine unexplained incident. It is not. As long ago as May 1977, my research revealing this "UFO" to have been nothing but Venus was reported in the *Washington Post*. When Hynek and I appeared on a network TV show in New York City in June of 1977, I found he was aware of my explanation for the case and thought it might very well be correct. Yet in this recording, which cites Hynek as its "director," the Carter UFO sighting is presented as if it were an unsolved mystery.

The recording also asserts that "ancient cave drawings of strange flying craft have been discovered by archaeologists"—in other words, "ancient astronauts." Now if you ask Hynek if he believes in ancient astronauts, he will say no. But this wild and ludicrous claim was permitted to remain. The Psychological Stress Evaluator (PSE) machine (see p. 44 of this issue), which supposedly determines truth from falsehood by the quivers in a person's voice, is touted throughout the recording as a highly accurate means of authenticating claims of supposed "UFO abductions." Hynek is heard endorsing the merits of the PSE, claiming that it indicates that Travis Walton is telling the truth about being on a UFO for five days (although Walton told me that he does not himself accept the validity of a PSE machine for such purposes). The PSE is said by the narrator to be rapidly replacing the traditional polygraph machine across the nation, because it is supposedly more reliable. (The *National Enquirer* has "proved" many bizarre claims using the PSE.) No mention was made of a 5-year-old study commissioned by the army that found that the PSE test had a 32 percent reliability rate (you can

achieve 50 percent accuracy by flipping a coin). A later study found a 38 percent success rate. A researcher at the University of Oregon reported that, in a test, while none of his students succeeded in beating a polygraph, nearly all of them fooled the PSE. At least Allan Hendry in CUFOS's *International UFO Reporter* had the good sense to warn against this record's over-reliance on the PSE. CUFOS's Hynek, on the other hand, embraced the PSE warmly.

Perhaps the most sensational topic to be dealt with on this recording is the widely believed "crashed saucer" allegation: that the U.S. government supposedly possesses, deep in the recesses of the Pentagon or elsewhere, the remnants of one or more flying saucers that have crashed, along with the pickled bodies of several humanoid aliens who piloted them. Until quite recently, such claims were believed only by the farthest-out contactee fringes. But recently the topic has become more UFOlogically respectable, and in this recording Hynek has at long last given at least a tentative endorsement to such yarns. Hynek states that, until recently, his reaction to such stories has always been "complete skepticism." True. "Recently, however," Hynek continues, "some of my colleagues, Investigative Research Associates, have probed into these stories, and some intriguing information has surfaced." Hynek observes that his contact with claimants to crashed-saucer information is "leading me to think that these people are indeed fearful to speak candidly about their experiences, whatever they might have been." He stops just short of a firm endorsement of the claim that there are little men in pickle jars in the Pentagon, although he is apparently giving such claims very serious consideration.

But the very idea of crashed saucers seems totally incompatible with the idea Hynek advances in the next section, that UFOs originate in a "parallel reality." He correctly criticizes the widely held notion of UFOs as extraterrestrial spacecraft, noting that the reports we receive are too inconsistent to represent anything so mundane as this. He does not explain how a visitor from a "parallel reality" might leave behind a crashed saucer with dead little green men strewn about. Nor does he attempt to reconcile the "parallel reality" pseudo-hypothesis with his firm endorsement of the "star map" that Betty Hill supposedly saw while being held aboard a UFO. Why would the visitors from Wonderland have need of a star map for interstellar navigation? (Betty Hill's "abduction" claims have been amply refuted in various places: see Klass, *UFOs Explained;* Menzel and Taves, *The UFO Enigma).*

The recording misrepresents one more significant fact: Benjamin Simon, the psychiatrist who hypnotized the Hills, was said to be "astounded" by the abduction stories told under hypnosis by Betty and Barney Hill, and he is portrayed as undecided about whether they really were taken aboard an alien craft. This is grossly incorrect. Dr. Simon has unambiguously stated on various occasions that he "absolutely" does not believe that the Hills were really "abducted"; he regards the story as a fantasy and not a real event. While there are enough arguments against the Hill case to fill entire volumes, not one of them finds its way into this "completely factual documentary."

The "first completely factual documentary on UFOs" will not be found here.

—Robert Sheaffer

Robert Sheaffer is a computer scientist and UFO investigator.

Psychic Vibrations

Bigfoot, America's favorite monster, of course needs no introduction. "Bighead" was reported in this column recently. Now "Littlefoot," said to be from "outer space," was reported along Mississippi's Gulf Coast. The *Gulfport Herald* reports that W. B. Phelps was awakened by a bright light near his trailer, from the direction of an uninhabited area. The next morning, he reportedly found in his yard a footprint that looked like a baby's print, except that instead of toes it had four spreading claws. (How old would Rosemary's Baby be now?)

* * * * *

For those of you who missed your chance to win the £1,000 award from Cutty Sark Scotch Whisky for the most "scientifically valuable" paper on UFOs, you now have another chance to win fame and fortune. (The Cutty Sark prize was won by CSICOP Fellow James Oberg's paper, "The Failure of UFOlogy," which prompted certain well-known UFO proponents to claim that the competition must have been rigged!) The *Meta-Science Quarterly,* which publishes scholarly sounding papers on far-out subjects, has announced the creation of an annual MSQ Award, giving a $100 prize each for the three papers showing the most "scholarly approach, scientific rigor, originality, and creativity" in areas of "consciousness related research." Papers are solicited on such scholarly topics as "clairvoyance," "mediumship," "life after life," "Lockness [*sic*] Monster," "Bermuda Triangle," "Pirimidology"*(sic),* "Tarot," and "Cabala." Send your entries to: Box 32, Kingston, Rhode Island 02881. Also in the *MSQ:* Dr. Carrol B. Nash, director of the Parapsychology Laboratory of St. Joseph's University in Philadelphia, suggests that the much-discussed search for extraterrestrial intelligence may best be carried out using ESP. He reasons that extrasensory communications need not be limited by language barriers and that they presumably travel faster than mere radio waves, which dawdle about the cosmos at the paltry speed of light.

* * * * *

Some astonishing developments were revealed in *Saga UFO Report's 1980 UFO Annual.* The previous year was trumpeted as a year of "astounding foreign encounters." Our favorite is the series of reports coming from the town of Xucurus, in Argentina, telling of sightings of "Martians" who are said to resemble "enormous portable radios." Also in that issue, paranormal

researcher Larry Arnold, who elsewhere has suggested that the nuclear accident at Three Mile Island was caused by telepathic sabotage, announced the discovery of a "Pennsylvania Triangle," a mysterious region where water *freezes* during the hottest part of the summer and then *thaws* as winter approaches. A "genuine" fragment of a UFO is also alleged to have turned up in this region, only to be forcibly confiscated by NASA, never to be seen again. The Pennsylvania Triangle must now take its place alongside such other scary triangles: Bermuda, the Great Lakes (says Jay Gourley), Tennessee (says the *Star),* Kentucky Bluegrass (says the *National Enquirer),* Ecuador (again the *Star),* and the Adriatic (say Italian newspapers).

* * * * *

A tempest in the UFO teapot is brewing over a new, richly illustrated book titled *UFO . . . Contact from the Pleiades!* The book reportedly depicts the "four-year ongoing extraterrestrial contact" of Eduard Meier of Switzerland, who claims to have had over 90 UFO contacts and to have taken over 800 photographs of supposed UFOs. A slick new book is being published in the United States on Meier's supposed encounters, containing color UFO photos up to 24 inches long, boasting of "actual quotes from the Aliens." This past September, L. James Lorenzen, international director of APRO, a major UFO group, mass-mailed a letter touting various UFO publications, including *Contact from the Pleiades* (offered "at a special APRO discount"). Lorenzen endorsed the book as containing "the best images of literally hundreds of startling photographs . . . what appears to be the most heavily documented series of UFO encounters—ever." But Meier's yarns went too far even for many UFO proponents, and Lorenzen quickly shifted into reverse gear. By December, he had belatedly noticed several possible flaws in the Meier account. For a start, it seems that Meier's alleged extraterrestrial contact had begun at age 4. Lorenzen also discovered that Meier had built many UFO models, which look remarkably like those in his photographs, and claimed to have photographed, in addition to UFOs, alien cosmonauts and a prehistoric pterodactyl (taken when the alien saucer took him back through the eons). Lorenzen says that his opinion is, and always has been, that the Meier case is an elaborate hoax. His earlier praise of Meier's photos, he now insists, was only meant to commend them as "art" and not to suggest in any way that they were authentic. It must have been those aliens from the Pleiades who mass-mailed that earlier APRO sales pitch.

* * * * *

A remarkable new development in UFOlogy was reported last November in the *MUFON UFO Journal,* published by one of the most prestigious UFO organizations in this country. But the dramatic development was "buried" on page 7 under the headline "Humanoid Enounters in Malaysia." In previous reports on the creatures that allegedly fly UFOs and that sometimes are claimed to abduct Earthlings for physical examinations aboard flying saucers, the "UFOnauts" are said to be approximately 4 to 5 feet in height, although a few stand only 3 feet tall. Now, from Malaysia, come a number of reports of UFOnauts that reportedly stand only 3 to 6 *inches* high, and whose spacecraft (understandably) are said to be similarly miniaturized,

according to the MUFON article. One incident, involving multiple witnesses, suggests that females have achieved full equality in at least one extraterrestrial society. The MUFON article reports a group of UFOnauts included both sexes and that the "females were notable because of their long hair."

Five of the seven "mini-UFOnaut" reports came from schoolchildren, and all the landings reportedly occurred on school premises, according to MUFON's West Malaysian representative, Ahmad Jamaludin. The Malaysian reports suggest that well-known UFOlogist Jacques Vallee could be correct when he earlier suggested that UFOs may be related to fairies and leprechauns.

The very next issue of the *MUFON UFO Journal* carried the following editorial by editor Richard Hall: "An appropriate New Year's resolution for all of us would be to 'clean up our act'—to be more thorough, and critical in our investigations of and reporting 'high strangeness' UFO cases in particular, and UFO reports in general. To some degree, both Allan Hendry [chief investigator of the Center of UFO Studies] and James Oberg [co-vice chairman of CSICOP's UFO Subcommittee and frequent author of *Omni* magazine's UFO column] are correct in their criticisms of rank-and-file 'ufology.' Both, however, paint with a broad brush and seem unable—or unwilling—to recognize competent work by others. Oberg, especially, uses the excesses of irresponsible or incompetent investigators to brand us all. Without considering motives, it must be stated that what they say is grounded in fact. Careless, incompetent, incomplete, uncritical investigation and reporting abound in the field. If we hope to be taken seriously, we must upgrade the quality of our work and set exacting standards for truth and accuracy." In the same issue, MUFON Director Walter Andrus disavowed the authenticity of Meier's *Contact from the Pleiades.* But on a facing page of the same issue, experienced UFOlogist and regular columnist Lucius Farish wrote that, regardless of whether the book's photos are genuine, "one must admire the perseverance of [Wendelle C.] Stevens and others who have labored hard and long to present this case to the public. Whatever one may think of the photos, the contact [with UFOnaut] claims or the other evidence, these research efforts should not be ignored . . . I would hope that this evidence will be given careful and serious consideration . . ."

* * * * *

Brad Steiger, one of the most prolific proponents of UFOs and the paranormal, has written his umpty-umpth book (we lost count at 100). Its title: *Encounters of the Angelic Kind.* According to Steiger, angels originate in some "invisible and non-physical realm or dimension or vibration," and selflessly assist earthlings in distress. Steiger's wife, Francine, is an authority on guardian angels in her own right, claiming to have had several encounters with a white-robed angel named Kihief. The tabloid publication *UFO Review* has a special offer should anyone want to throw money away on Brad Steiger's book on angelic encounters *and* Francine Steiger's learned treatise, *Reflections from an Angel's Eye.*

* * * * *

National Review reports that Calumet Township, Indiana, has launched an ambitious drive to eradicate all traces of voodoo from local government. One employee reportedly used "witchcraft" to induce a welfare recipient to

cooperate in a scheme to cheat the taxpayers, and this individual was reportedly found to be not the only sorcerer employed by the township. Because of this, Calumet has now banned the use of "voodoo, witchcraft, spiritualism, spells, or other mind-controlling techniques," at least during working hours.

* * * * *

Newsweek magazine carried in its issue of January 28, 1980, an item on the celebrated supernatural spoon-bender Uri Geller. This famous "psychic" revealed that he has approached a perfume company about developing a scent that can bring out paranormal powers in anyone who wears it. He also claims to be able to help mining companies locate underground ore deposits. Geller says he is writing a novel about a visitation to earth by an alien from outer space who has superhuman sexual powers. Geller also warns that within 50 years the Soviets will have developed such powerful psychic abilities that they will be able to knock out U.S. radars. (Whew! We were worried that they might perfect this within the next *five* years!) "I think we're entering an era where powers like mine will be believed," Geller says hopefully.

The editor and principal author of "Psychic Vibrations" is Robert Sheaffer.

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Superstitions: Old and New

William Sims Bainbridge and Rodney Stark

Ours is an age of science, yet it also is an age of superstition. Continuing vehement opposition to the theory of evolution by fundamentalists has aroused consternation within the scientific community. New cults promote modern forms of magic, and belief in paranormal phenomena is widespread. Rationality seems assaulted on the one side by the ancient myths of traditional religion and on the other by pseudoscience and novel occultisms. In this paper, we show the connection between the old superstitions and the new. We will find that our society is faced not with a choice between rationality and mysticism but, at best, with a choice of which style of supernaturalism science must endure.

The analyses that follow will use several sets of sociological data to evaluate the effect of traditional religion on attitudes toward supernatural and paranormal notions. We will demonstrate three specific points:

1. Among the general public, there is a strong tendency for members of conservative and fundamentalist religious groups to reject Darwin's theory of evolution.

2. There is a strong tendency for "born again" Christians to reject several occult and pseudoscientific notions.

3. Cults and individual occult activity tend to be most common where traditional churches are the weakest.

The first of these three points is no surprise, but the relationship between religion and rejection of Darwin deserves to be documented with precision. The second and third points show that the influence of traditional religion is complex, in some ways opposing science but in other ways defending rationality against several modern superstitions. Of all

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traditional religious beliefs, those antagonistic to the theory of evolution are most clearly antiscientific, so we will begin by examining them.

Surveys of Attitudes Toward Darwin

Many writers have described the fundamentalist campaign against the teaching of biological evolution in the schools, focusing on hard-core groups of creationists who constitute a small, though active, minority. We will show that there is vast opposition to Darwin by masses of ordinary citizens. While antagonism to other parts of science may also be felt by fundamentalists, it is the theory of evolution that receives the most concerted attack. Undoubtedly, this reflects the head-on competition between Darwin and Genesis. We must *not* assume, however, that fundamentalists reject science as a whole any more vehemently than do other groups.

One of the first really good surveys of religious beliefs was a study of Northern California church members carried out by Charles Y. Glock and Rodney Stark in 1963. A very long questionnaire was completed by 3,000 persons who belonged to a variety of denominations and religious sects. Recently, we have reanalyzed this massive data set (Bainbridge and Stark, forthcoming d) and found that even after 16 years it continues to reveal new insights. One of the questions asked was: "Do you tend to agree or disagree with Darwin's theory of evolution—which maintains that human beings evolved from lower forms of animal life over many millions of years?" Table 1 shows the percentages in each of a number of groups that completely rejected Darwin.

Table 1 is based on responses from 2,871 white church-members, and it shows a consistent pattern. Only 11 percent of those in liberal Protestant denominations rejected Darwin, and 29 percent in moderate denominations. Two denominations have many qualities of the more radical sects, the Missouri Synod Lutherans and the Southern Baptists. About two-thirds of their members say the theory could not possibly be true. Five other Protestant sects show high levels of rejection, all the way up to 94 percent of the Seventh Day Adventists.

As we scan Table 1 from the most liberal denominations to the highly conservative fundamentalist sects, we see steeply rising opposition to Darwin. Finally, the table compares the entire Protestant group with Catholics. The Protestant total includes 20 members of two sects that were too tiny for reliable separate statistics—the Four-square Gospel Church and the Gospel Lighthouse. Overall, the Protestants and Catholics show almost exactly the same levels of rejection, which suggests that a significant fundamentalist minority lies hidden within Catholicism.

TABLE 1
Rejection of the Theory of Evolution in the 1963
Survey of 2,871 Northern California Church Members

Church Membership	Percent who say that the theory could not possibly be true
Protestant Denominations	
1,032 liberal Protestants (Congregationalists, Methodists, Episcopalians, Disciples of Christ	11
844 moderate Protestants (Presbyterians, American Lutherans, American Baptists)	29
Sectlike Protestant Denominations	
116 Missouri Synod Lutherans	64
79 Southern Baptists	72
Protestant Sects	
44 Church of God	57
37 Church of Christ	78
75 Nazarenes	80
44 Assemblies of God	91
35 Seventh Day Adventists	94
Totals	
2,326 Protestants	30
545 Catholics	28

Several religious groups were not included in the Northern California survey. Sociologist Armand Mauss has provided us with data from his study of 958 Mormon residents of Salt Lake City and 296 Mormons living in San Francisco. His questionnaire included the same question about Darwin's theory used by Glock and Stark that was reported in Table 1. In Salt Lake City, where the Mormon church is the dominant religious group, 56 percent feel that Darwin's theory could not possibly be true. In San Francisco, where the Mormon church is a small minority, one finds a

greater variety of people who are or were members. Mauss surveyed 106 people who had left the Mormon church after having been raised in it, and only 13 percent of these defectors completely rejected Darwin's theory. There were also 67 persons who recently had been converted to the church, and just 28 percent of these new Mormons rejected Darwin. But of 122 lifelong Mormons, 59 percent felt the theory could not possibly be true. Thus, of people who have been committed Mormons for some time, who have long received Mormon teaching and continue to accept it, 56 percent or more reject Darwin absolutely.

TABLE 2

Agreement with the Theory of Evolution in the 1973 Survey of 1,000 San Francisco Area Residents

Religious Affiliation	Percent who agree that "Man evolved from lower animals"
Conservative Protestants (Missouri Lutherans, Southern Baptists, members of sects)	10
Moderate Protestants (Presbyterians, American Lutherans, American Baptists)	43
Liberal Protestants (Congregationalists, Methodists, Episcopalians, Disciples of Christ)	53
All Protestants	39
Catholics	38
Those with *no* religious preference	74
Agnostics and atheists	81

Note: The original study used a quota sample technique, so these figures are based on data adjusted to reflect the general population of the area.

Since the Glock and Stark study was a survey of church members, it did not include atheists, agnostics, or persons who were indifferent to religion. Recent research, also carried out in Northern California, updates the 1963 findings and extends them to citizens who are not church members. In 1973, a team of sociologists surveyed 1,000 residents of the San Francisco area (Wuthnow 1976). As we shall see later, the West Coast is the least religious region of the country, containing many who have only the slightest contact with traditional Christian influences. One question in the 1973 survey asked whether respondents agreed that "man evolved from lower animals." Table 2 shows the percentage accepting Darwin in each of several groups.

Only 10 percent of conservative Protestants agree with this simple statement of the theory of evolution. Fully 94 percent of this group accept the alternative view that "God created the first man and woman." Presumably, the 4 percent of conservatives who accepted *both* theories are especially agreeable people. Acceptance of Darwin rises sharply to 43 percent among the moderate Protestants and to 53 percent among liberal Protestants. Again, total figures for Protestants are almost exactly the same as for Catholics. Darwin is accepted by 74 percent of those with no religious preference and by 81 percent of agnostics and atheists.

The results in Table 1 and Table 2 should not be overinterpreted. It would be a mistake to conclude that fundamentalists oppose all science. Here, they have registered opposition to but a single theory, one that directly contradicts the Bible. But it would be an equally great mistake to conclude that religious liberals and the irreligious possess superior minds of great rationality, to see them as modern personalities who have no need of the supernatural or any propensity to believe unscientific superstitions. On the contrary, we shall see that *they are much more likely to accept the new superstitions.* It is the fundamentalists who appear most virtuous according to scientific standards when we examine the cults and pseudosciences proliferating in our society today.

The University of Washington Study

In 1979 we administered a long questionnaire to 1,439 students at the University of Washington in Seattle (Bainbridge and Stark, forthcoming b, c). Response to one question taken from a Gallup poll was particularly revealing: "Would you say that you have been 'born again' or have had a 'born again' experience—that is, a turning point in your life when you committed yourself to Christ?" In 1977, Gallup found that 34 percent of American adults claimed to be "born again," an indication that conservative Christianity remains strong in our society. One might think

that undergraduate students in our university would be modern, secular persons. But 26 percent said they were "born again." Of course, some of these students may have been lapsed Christians who had abandoned conservative religion after having been "born again" some time in the past. Therefore, in our analysis we focus on the 245 students who said they had had the "born again" experience and also said their religious beliefs were currently "very important" to them.*

After removing all the "born agains," we were left with 251 ordinary Catholics and 319 ordinary Protestants. Another 241 respondents claimed to have no religious affiliation. We will not consider the Jewish students here, nor the members of numerous "other" groups, because they represent very small minorities in this sample.

Before analyzing occult and pseudoscientific opinions, we need to understand something more about the ordinary Catholics and Protestants. These young people are not necessarily members of churches. For many of them, religion is an unimportant detail of their family backgrounds. One indicator of this is the fact that many Protestants could not spell the names of their denominations! Several claimed to be "Lutherns," while others were "Piscables" or "Presbitarians." Perhaps the best quantitative measure of real religious involvement is church attendance. Not surprisingly, zero percent of those with no religion attend church every week. Among ordinary Protestants, only 5 percent attend weekly, as do 25 percent of ordinary Catholics. Among the 245 "born agains," 68 percent attend church at least once a week. Thus, while the "born agains" do appear to be a profoundly religious group, the ordinary Protestants and Catholics include many whose religion is only nominal and may have no influence on their lives.

We included an evolution question in our student survey, the one used in the 1973 San Francisco area study. Only 20 percent of the "born again" university students agreed that "man evolved from lower animals," showing that conservative religious opposition to Darwin is strong even among the student body of a leading secular university. Almost equal percentages of ordinary Catholics and ordinary Protestants agree with the

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*Several earlier research studies, including some by each author of this article, failed to find a significant impact of religion on patterns of deviant behavior. We now realize that those studies simply did not include enough highly committed members of cohesive fundamentalist groups to show the results reported here. As this article demonstrates, data covering the entire nation do reveal a strong religious influence against occultism. Research performed in the Pacific region, where religion is very weak, might miss this effect unless it includes individuals, like "born agains," who are involved in an intense religious social movement that counteracts the regional irreligiousness. In the parts of the country where religion is strong, even members of moderate denominations may be protected from occultism by their religion.

statement, 74 percent and 76 percent, respectively. The highest level of acceptance, 93 percent, is found, as expected, among those with no religion. Before we leap to the false conclusion that the students with no religion are rational, scientific secularists, we should look at Table 3, which reports the distribution of responses toward some recently popular superstitions.

TABLE 3

Attitudes Toward the Occult in the 1979 Survey of University of Washington Undergraduates

	Percent Giving the Indicated Response			
	241 with no	Ordinary Christians 251	319	245 Born Again
Questionnaire Item	religion	Catholics	Protestants	Christians
Agrees: "Some Eastern practices, such as Yoga, Zen, and Transcendental Meditation, are probably of great value"	73	66	60	28
Agrees: "UFOs are probably real spaceships from other worlds"	67	66	60	43
Agrees: "Some occult practices, such as Tarot reading, seances, and psychic healing, are probably of great value"	16	22	12	6
Agrees: "I myself have had an experience that I thought might be an example of extra sensory perception"	59	57	55	44
Respondent thinks that ESP "definitely exists"	26	29	17	17
Respondent very strongly dislikes "occult literature"	38	34	36	65
Respondent very strongly dislikes "Your Horoscope"	29	24	23	53

The first four items in Table 3 come from a list of statements to which each of the students was supposed to respond by checking one of four boxes: strongly agree, agree, disagree, strongly disagree. We did not give students the opportunity to say they were "not sure" or "undecided" or had "no opinion." Among those who checked the "agree" box are undoubtedly many who were just barely willing to give the particular statement the benefit of the doubt. Therefore, it may not be valid to take the figures as exact measures of *actual belief* in each statement, but it is valid to compare *levels of support* for each idea.

The first statement refers to Yoga, Zen, and Transcendental Meditation, practices alien to Western traditions but deeply rooted in Eastern religion. Seventy-three percent of those with no religious affiliation are willing to say that these oriental cults are probably of great value; 66 percent of the ordinary Catholics and 60 percent of ordinary Protestants agree. Thus, those with no religion or only nominal religion are especially likely to accept deviant, exotic alternatives to Christianity, just as they are likely to accept Darwin. Apparently, they are open to new ideas of many kinds, rather than accepting Darwin because they are well-informed secular rationalists. Among the "born again" Christians, only 28 percent express positive evaluation of the oriental cults.

The statement that UFOs are probably real spaceships also receives much stronger support from those with no religion or only nominal religious affiliation. The percentage of "born agains" accepting the statement is higher this time, perhaps reflecting the fact that belief in UFOs does not necessarily have religious implications. But only a minority of "born agains" think UFOs might be spaceships, compared with clear majorities in the three other groups.

Deviant magical practices, such as Tarot reading, séances, and psychic healing, are very unpopular. But even at this extreme, "born agains" show by far the lowest level of acceptance.

One might predict that religious people are more likely than the irreligious to believe in extrasensory perception. For one thing, people who believe humans have souls that transcend the material world already share some of the assumptions behind belief in ESP. Furthermore, guidance from God, and even prayer to God, would seem to be communications by the spirit rather than by the senses. Two questionnaire items about ESP show only slight differences among the four groups of respondents, but in both cases those with no religion are significantly more likely than "born agains" to favor the idea of extrasensory perception.

Part of the questionnaire consisted of a long list of things (ranging from foods to movies to other consumables and mass media) and asked students to indicate how much they liked each one on a seven-point scale

from "0" (do not like) to "6" (like very much). Table 3 concludes by comparing the percentages who gave zero ratings to "occult literature" and to "Your Horoscope." In both cases, "born agains" were nearly twice as likely as other students to reject these forms of occultism.

As a whole, Table 3 shows two very interesting things. First, it reveals that "born agains" are much less likely than other students to accept radical cults and pseudoscientific beliefs. Second, it reveals that the group with no religious affiliation is receptive to these unscientific notions. On three of the seven items, in fact, those with no religion are the most favorable toward occultism. Those who want to blame the fundamentalists for their opposition to Darwin ought to praise them for their responses reported in Table 3. Those who hope that a decline in traditional religion would inaugurate a new Age of Reason ought to think again. Having seen results from three large questionnaire surveys, we will now examine the relationship between religion and the occult through geographic analysis.

Geography of the Supernatural

Working from an incomplete census of religious organizations carried out in 1971, Stark (forthcoming) has been able to calculate good estimates of the total number of church members in every state and many major cities. For our present purposes, the raw figures are not very revealing, so we have calculated *rates* of church membership per thousand residents in each geographic area. Figure 1 gives these rates for the nine regions of the United States, defined by the Bureau of the Census. Nationally, there were about 115 million church members in a total population of 205 million, for a rate of 560 church members per thousand. As Figure 1 shows, the nine regions vary considerably in the proportions of citizens who formally belong to a church.

Seven of the nine regions have church-member rates at or above the national average. In the West South Central region, nearly two-thirds of the residents are members of a church. The remaining third are not atheists, of course, but mostly unchurched Christians who profess religious belief but have no continuing participation in a religious group. The vast, but thinly settled, Mountain region has a rate of 530 per thousand, slightly below the national rate of 560. But the main feature of this map is the Pacific region, which includes Alaska and Hawaii, as well as California, Oregon, and Washington. Here, the church-member rate is 360 per thousand, 200 less than the national rate. Again, this does not mean that the majority in the Pacific region are atheists but that the majority are unchurched Christians. But whatever people's private religious faith, the rates shown in Figure 1 do measure the *strength of organized religion.*

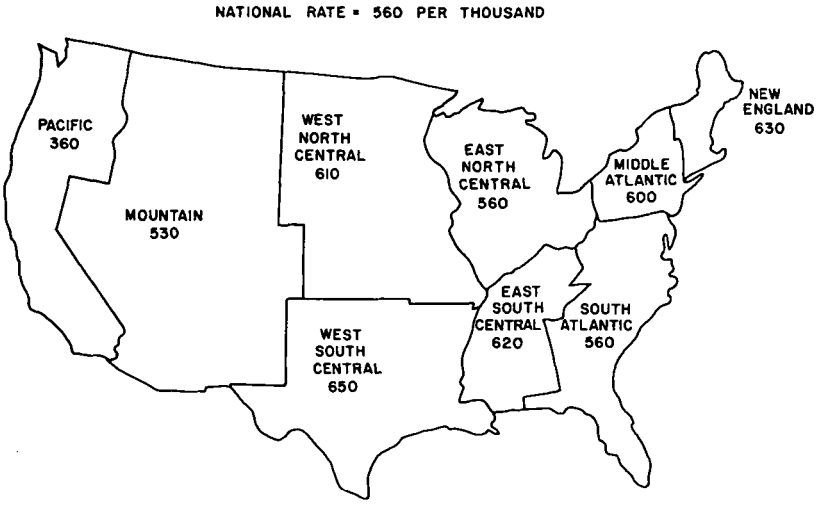

FIGURE 1. Church membership across the nation in members per thousand residents

This is not the place to explain all the religious trends of recent years, but we should point out that the Pacific rates do not necessarily reflect any greater rationality in the West. We think the weakness of conventional religion in the Pacific region may be due to nothing more subtle than the migration patterns in this area. Preliminary research by our associate Kevin Welch suggests that geographic mobility (moving from one home to another) often tears people away from one church community without placing them in contact with a united group of new neighbors who will invite them to join a specific conventional church near their new home. Regions with high rates of migration will therefore show low rates of church membership. We are still engaged in research on the causes of defection from the conventional churches; but certainly much of it flows from sources other than personal convictions opposed to religion, and many among the unchurched want contact with the supernatural.

Our questionnaire research suggests that strong religion prevents occultism. Therefore, we would expect to find that interest in deviant cults and in the paranormal was greatest where the churches are weakest—in the Pacific region. In fact, this is the case. Perhaps everyone interested in cults already knows that Southern California is a hotbed of them. But our research has shown that cults and occultism are strong all the way up the coast, through Oregon, Washington, British Columbia, to Alaska (Stark, Bainbridge, and Doyle 1979; Bainbridge and Stark, forthcoming a). Let us look at some of the evidence.

Recently, we have developed several measures of cult activity that can be analyzed geographically. One such is a list of 501 headquarters of cults

drawn from J. Gordon Melton's monumental *Encyclopedia of American Religions* (1978). The cults are of many kinds, including New Thought, Theosophist, Spiritualist, psychedelic, psychic, pagan, witchcraft, satanic, flying saucer, occult, and communal groups. California is the headquarters for 167 cults, a third of the total, proving its reputation as cult capital of the nation. But less populous states in the Pacific and Mountain regions also have more than their share of cults. The 167 in California yield a rate of 7.9 per million population. Sparsely inhabited Nevada, with only 6 cults, achieves a rate of 10 per million. New Mexico has a rate of 9.1 per million. Colorado has 6.0, Arizona 5.9, and Oregon 4.8. There are 192 cult headquarters in the Pacific region, for a rate of 6.6 per million, nearly three times the national rate of 2.3. Figure 2 shows cult rates for the nine geographic regions.

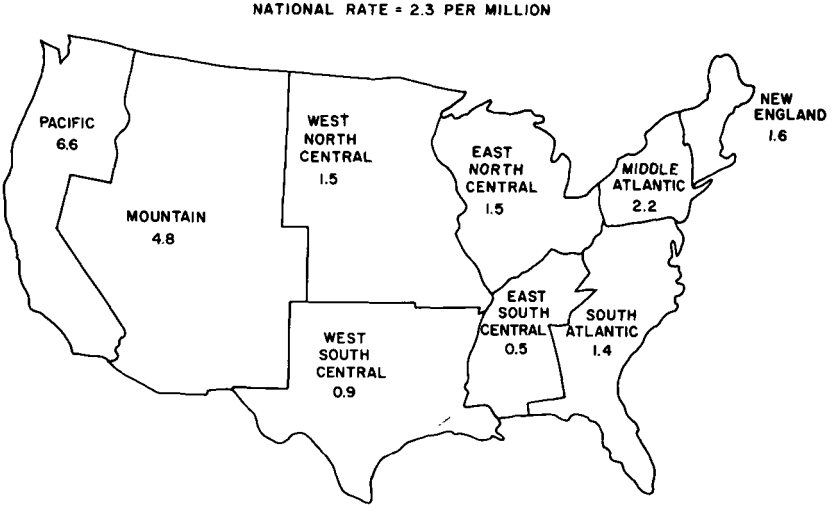

FIGURE 2. Distribution of 501 cult headquarters in cults per million residents

Figure 2 demonstrates that cult headquarters do indeed cluster where the conventional churches are the weakest—on the Pacific shore. Apparently, when Christianity loses its grip on large numbers of people, deviant religious alternatives arise and get hold of some of the unchurched. There are two ways in which this process of transformation takes place. First, strong traditional churches can use their influence in the community to punish involvement in deviant supernatural activities; but when churches lose their power, people are liberated to follow their religious enthusiasms. Second, religion serves important social and psychological functions for many people and, when the conventional churches fail in this task, people will seek satisfaction from other sources. This means not only

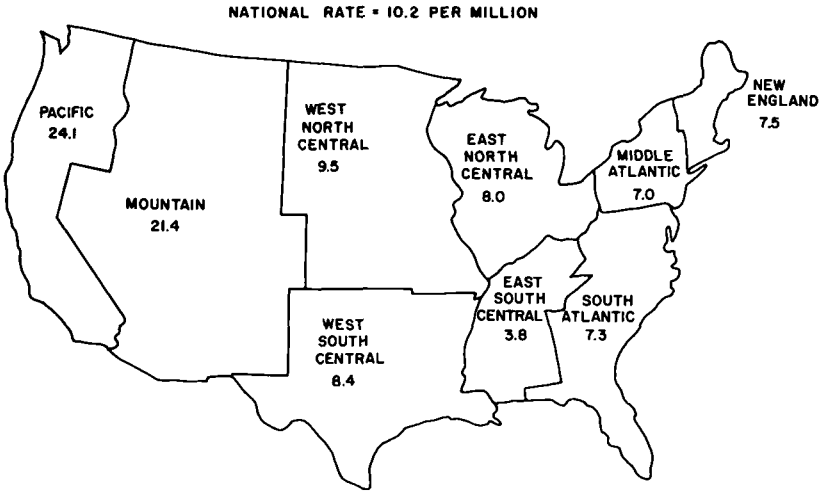

FIGURE 3. Distribution of 2086 Fate Magazine readers' stories given in stories per million residents

that many will join formal cult movements but also that others will seek personal contact with the supernatural realm. An excellent indicator of this latter course is found in the pages of *Fate* magazine.

Fate is a central medium of communication for the American occult, reaching far more than 100,000 readers with each monthly issue. Among the standard features are columns of readers' stories, submitted for token rewards, supposedly proving the existence of supernatural events and beings. Founded at the beginning of 1948, *Fate* quickly began the first of these, called "True Mystic Experiences," a potpourri of paranormal reports. In 1954, *Fate* added "My Proof of Survival," stories of apparitions, near-death experiences, and messages from the beyond. From January 1960 through September 1979, *Fate* published a total of 2,086 of both types by authors with American addresses. Figure 3 shows the distribution of the stories per million population for each of the nine regions.

The rates shown in Figure 3 are higher than those in Figure 2, simply because it tabulates more than four times as many items. But the shape of the distribution is almost identical. The Pacific region is highest. The Mountain region comes second. Other regions fall near the national average, except for the very low East South Central region. There is no direct connection between these two measures of cult activity. We have no reason to believe that any significant number of the 2,086 stories were written by members of the 501 cults. Rather, we think they are individual testimonials in favor of occultism, both permitted and stimulated by weakness in conventional religion.

Conclusion

Traditional religion is not simply the enemy of rationality and science, but it plays an ambivalent role. True, fundamentalists show high levels of rejection of the theory of evolution. But they also reject a wide range of occult or pseudoscientific ideas that may threaten the progress of human culture. Persons with no religious affiliation are often among the first to toy with novel or exotic supernatural notions and are not the secular rationalists we might want to think them. Cults flourish precisely where the conventional churches are weakest, in the western parts of the country. Here, too, numbers of unchurched people seek private contact with the supernatural, as shown in the distribution of *Fate* magazine "mystic experiences" and "proofs of survival." Therefore, a further decline in the influence of conventional religion may not inaugurate a scientific Age of Reason but might instead open the floodgates for a bizarre new Age of Superstition.

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Psychic Archaeology

The Anatomy Of Irrationalist Prehistoric Studies

Kenneth L. Feder

As a scientific enterprise, archaeology is relatively young. The use of objective, consistent, and meticulous field-techniques is barely 100 years old. As recently as 25 years ago archaeological theory—that is, a body of general theory explaining the nature of the relationship between human societies and the material remains of these societies—was so deficient that those who discussed it could state that it lacked even a name (Willey and Phillips 1958, p. 5). In the past twenty years archaeology has experienced a methodological and theoretical revolution characterized by the explicit application of statistical analysis, use of computers, a heavy emphasis on ecological relationships, and the growth of cultural evolutionary theory.

Though scientific or anthropological archaeology is new, archaeology has never lacked fringe-area, pseudoscientific, and, at times, decidedly *anti*scientific approaches and theory. Archaeology and astronomy are probably the two sciences that have attracted the greatest number of serious and dedicated amateurs who go on to make valuable contributions. These two fields have also attracted the greatest number of individuals whose time would be better spent selling incense. It is truly mind-boggling to consider the number of frauds (Piltdown Man, the Cardiff Giant, the Davenport Stones), racist ideologies (Nazism), religions (Mormonism), and just plain crazy theories (ancient astronauts) that have utilized archaeology to "prove" their often preposterous hypotheses.

It should come as no surprise that a "new" field of endeavor involving archaeology and the paranormal has been introduced: "psychic archaeology." After all, what could be more obvious? Psychics can find missing things; why shouldn't they be able to find archaeological sites?

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They can, for instance, psychometrize an object—that is, they can perceive detailed personal information about the owner of an object simply by holding it in their hands. Why shouldn't they be able to do the same for a 10,000-year-old spear-point?

It is not surprising that people who have embraced the paranormal would also wish to embrace psychic archaeology. If this was the extent of this phenomenon, it might best be ignored. However, psychic archaeology has gained an unfortunate professional tolerance, if not acceptance, in some quarters. Ivor-Noel Hume, one of the most influential (and, I might add, theoretically conservative) historical-site archaeologists in North America, details in his introductory text the technique of dowsing for the location of buried metal artifacts (Hume 1974, pp. 37-39). In a popular new introductory text, David H. Thomas, who is highly respected, and deservedly so, devotes a two-page inset to psychic archaeology and straddles the fence on the issue (Thomas 1979, pp. 286-287). However, far and away the most disturbing work is that of David E. Jones (1979), a professional anthropologist at the University of Central Florida. His book *Visions of Time,* published by the Theosophical Society, purports to be a "test" of psychic archaeology. Jones is honest enough in describing his interest in parapsychology as stemming from his desire to "escape from the tyranny of a Western 'logic' of the universe." Yet, through procedures that, at least superficially, feign Western scientific methodology, Jones attempts to test the validity of the psychic-archaeology hypothesis and states, "I feel a positive conclusion is unavoidable." Let us see just how "unavoidable" this "positive conclusion" is in the testing of psychic archaeology. Because Jones's work claims to be an objective test, and since Jones is a professional scientist, his work demands professional attention.

In essence, Jones tests some basic claims made by "psychic archaeologists":

1. the ability to locate unknown archaeological sites;
2. the ability to ascertain the location of buried artifacts and features at archaeological sites; and
3. the ability to perceive detailed information of both a general and specific nature about the prehistoric environment, culture, and even the emotional and physical states of individuals who have been dead for thousands of years, by simply handling artifacts. (As Jones states, "The psychic's claim, if it could be demonstrated *rigorously enough* [emphasis mine], was precisely what archaeology needed to make it an historical tool capable of generating cultural descriptions comparable to primary or first-hand observation" (Jones 1979, p. 4).

Let us see exactly how these phenomena are supposed to work.

Site Location

Whether lecturing to a class of archaeological neophytes or speaking to a local civic group, archaeologists are always asked how they find sites. The question is asked, at least initially, much in the same way one might inquire how pirate treasure is found. The notion is that sites are terribly rare, randomly strewn across the countryside, and almost impossible to find without detailed technical knowledge. With this in mind, certainly if a psychic could precisely locate a site it would seem to be impressive evidence that such abilities exist.

I am certain that it would not be giving away any trade secrets to admit that, although our methodologies may be quite sophisticated, involving rigorous sampling techniques, infrared photography, and so on, the theory behind locating archaeological sites is, in fact, quite simple. Human beings do not distribute themselves randomly across the landscape. Especially in technologically unsophisticated societies, people settle areas differentially, as a function of certain rather obvious environmental parameters. Among these are:

1. distance to potable water
2. distance to a navigable waterway
3. gentleness of topographic relief
4. defensibility
5. natural protection from the elements
6. soil type
7. presence of natural resources (stone quarries, clay, etc.)

Thus, while our methods for assessing the probability of site location in a given area are becoming increasingly sophisticated—Thomas (1972, 1973) utilized a computer simulation model—the gross variables considered are often commonsense characteristics. In order to survive, people need fresh water, a flat area large enough to accommodate the community, and so on. How much detailed technical information must someone have before being able to duplicate the accuracy of an archaeologist in finding a site? In many cases, not much.

In my introductory course in archaeology each semester, I distribute copies of United States Geological Survey quadrangle maps (1:24,000 scale) of an area of Connecticut that has been partially tested in the Farmington River Archaeological Project (FRAP). The goal of FRAP is to explicate, as well as explain, prehistoric human settlement in the Farmington River valley, a small to medium-sized tributary valley of the Connecticut River located in the central portion of the state. In this survey, test pits were excavated at 30-meter intervals along randomly selected transects running perpendicular to the river. Each transect cut across the

three distinct topographical/depositional zones in the valley floodplain, glacial terrace, and bedrock upland. A large number of prehistoric settlements have been located in this fashion. The vast majority of sites have been situated in the kinds of areas in which one would expect to find human settlements.

After only an hour or so of discussion of human settlement locational choice, where the students suggest the key parameters of this question and with no other information provided, I ask them to suggest areas where sites might be expected. Invariably, in a class of 40 students, a number point out *precise* locations where, unknown to them, sites have already been found by FRAP. Are these students psychic? Of course not. They simply paid attention and applied the reasoning that was discussed. To predict that an area where a swift brook enters a navigable river would have been attractive to human settlement is like predicting there will be cold nights in Alaska this January. You can't miss.

A perfect example of this "can't miss" test of psychic archaeology occurred in a recent program in the "In Search Of" series on one of the major TV networks. Here we are told of a fabulous experiment in underwater psychic archaeology where a group of "sensitives" were asked to identify the location of possible historic shipwrecks from sea charts. The areas suggested by the psychics were examined in a research mini-submarine, and, sure enough, a wreck was found, although not in any of the precise locations predicted by the psychics. The fact that a wreck was spotted (after many hours of searching) was presented as proof positive of the existence and utility of psychic underwater archaeology. The fact that the area chosen for psychic examination was historically one of the most highly traveled regions of the world's oceans, and where a number of wrecks were known to have occurred, was played down in the show.

According to Jones (p. 15), a number of key archaeological discoveries made in the past were based on the psychic abilities of the researchers. He very clearly implies that it was by using their "intuitive" (read *psychic*) powers that Winckelmann discovered artifacts at Pompei; Stephens and Catherwood, the Mayan civilization; Boucher de Perthes, paleolithic tools; Layard, the palaces at Nimrud in Assyria; and Schliemann, the ruins of ancient Troy. With the exception of Layard, who apparently did claim to possess clairvoyancy, these claims are extremely misleading. When a scientist, after years of analysis and meticulous, painstaking research, puts the data together in a new way, makes a discovery, and gains a new insight into a perplexing question, it is called rational thought, or deductive reasoning. There is no reason to call it paranormal.

Does Jones go on to adequately test the hypothesis that psychics can

"locate" sites? Not only does he not *adequately* test the hypothesis, he doesn't test this at all! At no point is a map presented to a psychic to predict the location of an unknown site whose existence can then be tested. Instead, Jones presents psychics with artifacts and photographs from *known sites,* whereupon they are asked to determine the location of these sites.

In one such experiment Albert Bowes (a professional psychic) was left *alone* in a room with his wife and a series of artifacts (p. 55). Bowes correctly identified the site as a coastal occupation, a result which is seen as impressive; after all, the site could have been in the mountains or in a desert. However, the fact that among the artifacts provided were shells and some columella beads may have given Bowes a tiny hint.

In another test Bowes was mailed four photographs of a Florida site, which he kept for three days, and was asked to provide locational data (pp. 106-114). Jones claims that the photographs are not diagnostic of Florida. This seems like a particularly poor way to test a hypothesis. Though Bowes correctly identified this and other Florida sites as being near "water," we must remember that (1) almost all prehistoric, as well as historic, habitation sites everywhere in the world are near water; (2) Florida has, in a state covering 58,560 square miles, approximately 1,800 miles of coastline (more than 8,000 miles of detailed shoreline); and (3) when a psychic correctly guesses that a Florida site is near a lake and even suggests the direction of the lake from the site, it is not terribly surprising, since there are approximately 30,000 lakes in the state. It would be difficult for a site in Florida not to be near a lake. Furthermore you can point in almost any direction from an archaeological site in Florida and find a lake nearby.

What evidence is given that psychics can locate unknown archaeological sites? In a word, *none.*

Site Excavation

In the excavation of most archaeological sites, researchers are faced with the problem of deciding precisely where to dig. All but the smallest of archaeological sites are too large to excavate in their entirety. Archaeological science deals with this problem through the application of rigorous sampling strategies designed to ensure a representative sample of the subsurface materials. Archaeological pseudoscience deals with this problem through clairvoyance.

Jones conducts four experiments purported to test psychic abilities applied to archaeological excavation. The bulk of the test results, however, have nothing whatsoever to do with the proposed psychic ability to

perceive unseen artifacts. Rather, most of the information provided by the psychics deals with reconstructing past cultures, that is, "sensing" what went on at a site when it was occupied. I will assess Jones's tests of the psychics' abilities at site reconstruction in the next section of this paper.

However, there were some attempts made by the psychics at specifying the material that was found, or would be found, at a particular site on the St. John's River in central Florida. James Randi has introduced the concept of "negative success" as a common rationalization used in psychic research—"when you win, you win; and when you lose, you win" (Randi 1975, p. 8). Jones exploits his own version of this when he states (p. 101): "If the psychic subject stated that a particular artifact was present in the site, the lack of its discovery during excavation would not disprove the presence of the artifact." This statement, while absolutely true, renders this set of experiments entirely meaningless, since a psychic *cannot* be proved wrong. Anything guessed at correctly is a hit. Anything guessed at which is not found does not *prove* a miss; it may merely mean that it has not yet been found. Philosophers of science often judge the utility of a hypothesis by its falsifiability. This particular hypothesis does not meet this criterion in Jones's test.

In terms of the actual testing, a specific list of artifacts found at the site is provided (p. 105). Of all the materials listed, Bowes was correct only twice in his psychic assessment of the artifacts that were present: a single iron nail and some metal pieces (all of non-Indian manufacture and intrusive to the site). The 11 projectile points, 42 flint pieces, fire hearths, shell tools, beads, worked bone, flat (milling?) stone, European ceramics, Indian pottery, and the human burial were not mentioned by Bowes in the first experiment.

Jones attempts to convince the reader that Bowes came close to perceiving the burials; Bowes did not. Instead, he talks about broken necks and heads on poles, but this is meaningless in testing the hypothesis. In fact, he initially states, "I really feel as though this site or area may not show that many bones or people" (p. 113). In the second experiment, where Bowes actually visits the site, he goes so far as to say "if you find skulls here, it would be surprising" (p. 115). Well, they did find a skull. Jones, of course, sees this as verifying Bowes's psychic abilities because, after all, *only* one skull was found.

The most abundant artifact found at the site was pottery, with over 5,000 shards recovered. Bowes, however, never mentions pottery at all until he is brought to the site and sees or is given a potsherd (in the test it is unclear where it came from). This is not a very impressive degree of accuracy.

Site Reconstruction

The claim is made that psychics can provide detailed information about prehistoric groups. In the lexicon of pseudoscience, "vibrations" of individual people and their natural and cultural environment are imprinted on their possessions, bones, and so on, and these "vibrations" can be read thousands and even millions of years later by psychically inclined individuals. According to Jones, what these people can accomplish is "like nothing that any archaeologist could do by simply observing the artifact" (p. 4). I shall ignore for the moment that archaeologists do a bit more than "simply observe" artifacts.)

The bulk of Jones's book is devoted to testing experimentally the psychics' abilities to accomplish this feat: "psychometrizing" an artifact, that is, psychically producing as detailed a description of prehistoric people as an ethnographer could who lives with a contemporary group. All archaeologists admit a deficiency in detail of our cultural reconstructions compared with those of the cultural anthropologist, who observes and interacts with a people in their daily lives. Can psychics recapture this lost detail?

A slight digression must be made here to discuss the methodology employed by Jones in his testing of this hypothesis. The essence of any good experiment, archaeological or otherwise, rests in consistency and objectivity. It is incumbent upon the rational scientist to detail, as explicitly as possible, the problem being addressed, the methodology and operationalization of the experiment to be performed, and, quite important, the precise nature of the results required to accept or reject the hypothesis. Jones goes as far as the first two steps but never discusses the third. We are not told beforehand what exactly will constitute proof or disproof of the hypothesis that psychics can provide detailed archaeological information. In any acceptable test of a hypothesis, it is necessary to present a set of predictions, to spell out precisely those things that *must be true* if the hypothesis is to be considered valid. It makes no sense, in an objective test of a hypothesis, to propose a phenomenon, design an experiment to test the phenomenon's existence, conduct the experiment, and then seek to explain all experimental results within the context of the assumption that the hypothesis is, in fact, valid and true. This constitutes post-hoc rationalization. This constitutes precisely Jones's "reasoning" in *Psychic Archaeology*. Post-hoc rationalization is *not* science. All of the results of the experiment, including those that quite clearly contradict the hypothesis, are interpreted, squeezed, fudged, and rationalized away as actually supporting the hypothesis. Scientists set out to *test*, not *prove* hypotheses. The trouble with trying to *prove* something

rests in the self-fulfilling prophecy: if you want to prove something badly enough, you always will.

Further, Jones states that he does not use quantitative or statistical techniques in the analysis of his data; that his discussion in the body of the test constitutes the "hits," rather than the misses, of the psychics. His reasoning seems to be that, while the psychics were *so* wrong part of the time that not even a post-hoc rationalizer could account for it, when they were correct it was so impressive as to alone prove the existence of psychic powers. Jones seems to believe that explicit, admitted avoidance of quantitative techniques obviates all criticism of his not being statistical. This is akin to a surgeon trying to avoid responsibility for the consequences of not sterilizing his or her instruments on the grounds that he/she specifically said it would not be done. That simply is not a good enough argument. The abandonment of statistical analysis by a person with an advanced degree in anthropology is appalling. This critical problem can be explained through the following example.

Suppose you designed an experiment to test whether or not a particular coin was "fixed." In the experiment you performed 100 sets of 10 flips each. Obviously, if the coin was legitimate you would expect (predict) that in the majority of sets of 10 flips you would obtain 5 heads and 5 tails. Statistically you would expect to achieve 10 straight heads or tails $1/2^{10}$ or 1/1024 times. With 100 sets of these 10 flips, your probability increases to 1/10.24. Thus, though it is unlikely, it is certainly possible. Suppose, when presenting the results, the coin tester stated: "In the 100 sets of 10 flips each, there were many nonsignificant (5 heads/5 tails) results, but let's forget these. On one occasion I obtained 10 consecutive heads—a highly unlikely result. I therefore conclude that the coin is fixed." This would not be accepted as being valid by any rational individual. This reasoning displays a complete lack of understanding of the meaning of "proof" in a statistical sense. This is precisely the approach used by Jones.

A key element in the statistical reasoning that *must* be used when testing probabilistic phenomena (which all psychics claim psychic phenomena to be) is the recognition that unlikely events *do* occur. The point of a statistical test rests in providing a mathematical measure of the likelihood of a given result in order for us to determine whether or not the result is so unlikely as to reject or accept the hypothesis.

Specifically, it can be certainly stated that none of Jones's quasi-experimental tests renders a conclusion in favor of psychic archaeology "unavoidable."

Jones initially ran a two-stage preliminary experiment. The first stage involved a double-blind reading of wrapped and boxed artifacts. Remember, Jones is giving a detailed report only of the *hits*. The claim is

made that Bowes scored positive hits on 50 percent of the 10 items he was presented. The case for the antique baseball in box number 4 is typical. Indeed, on page 20 a section of the transcript of the experiment is quoted in which Bowes mentions that the object in box number 4 pertains to a game and that the thing is a ball. Sounds impressive. Well, when we turn to the full transcript we are able to isolate 15 different specific references to the item in box number 4: (1) teeth, (2) pottery, (3) game, (4) ball, (5) Incas, (6) South American, (7) man with scar or injured jaw, (8) Egypt (9) mummy, (10) building, (11) cave, (12) pyramid, (13) supreme ruler, (14) bones, and (15) jaws. Hardly an extraordinary outcome.

The second part of the preliminary experiment involved a second reading, with the objects now open for view. Interestingly, in almost every case Bowes provides a far more detailed reading of the objects when seen than when they were boxed. Why would *seeing* an object that was being analyzed by another "sense" help in its description? Does seeing a stereo speaker help you hear it? Can you feel heat or cold better with your eyes open than with them closed? I suggest that it's just easier to make up a story about something if you can see it. The length and detail of the second set of psychic observations are far more congruent with this conclusion than any other I can suggest.

In one of the more purely archaeological experiments Bowes was given a set of artifacts and asked to analyze them. These items were from Lindenmeier, a Paleo-Indian site in Colorado. Jones could not have picked a worse example on which to run a test. The Paleo-Indian period, with its easily recognizable "fluted" points, is known to anyone who has seen even a child's book on Indians or American archaeology. During a recent lecture to fourth-graders I was asked about these "fluted" points, which are found across the country and date from 12,000 to 9,000 years ago. Certainly if I were Bowes, and even if I believed I was a psychic archaeologist, I would at least pick up a book or two on archaeology to get acquainted with the field. Anyone who did so would have to be unconscious not to come across at lease a brief discussion of these late Pleistocene inhabitants of the New World.

Even given all this, how well did Bowes do? Not especially well. He locates the site in California, New York, Nevada, and even suggests Georgia and Tennessee, though he correctly prefers the general area of the American west. He correctly identifies the period generally as being before Christ and before Egypt, but he never gets beyond this in specificity. Much of the other information provided by Bowes is problematical, vague, and, in general, meaningless. However, there is one point at which Bowes clearly gives away the source of his meager knowledge of Paleo-Indians. It is decidedly not psychic.

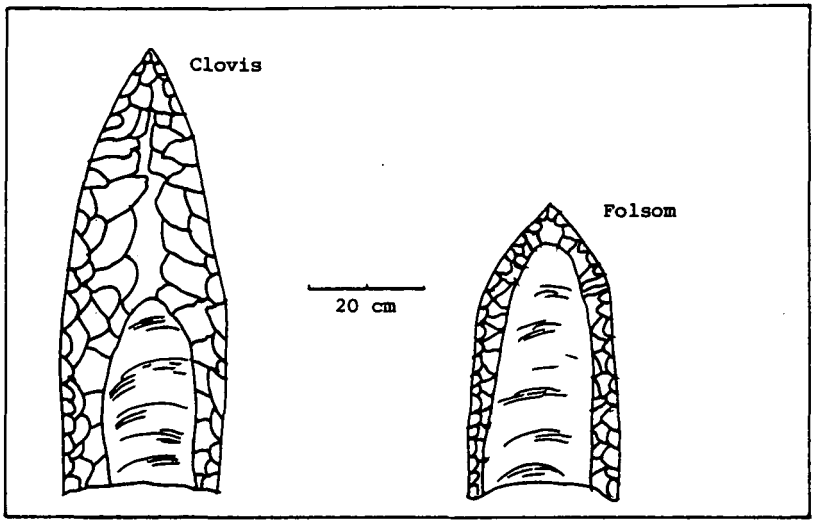

FIGURE 1. The two types of fluted points found in North America

When discussing diet and hunting techniques Bowes states that the fluted points at Lindenmeier were used "to shoot bison, or an elephant or mastodon or something" (p. 82). He later denies the presence of bison (which is rationalized by Jones to be confusion over the fact that an *extinct* form of bison was present at Lindenmeier). While most popular books on North American archaeology will tell you that Paleo-Indians hunted bison and mastodon with their fluted points, most neglect a small, technical piece of information. There were actually two forms of fluted points: Clovis points—long, leaf-shaped points with a channel on both faces rarely reaching beyond the midpoint of the projectile's length; and Folsom points—somewhat shorter leaf-shaped points with channels reaching nearly the entire length of the points (see Figure 1). The Clovis points, which tend to be older, are almost always associated with the remains of extinct proboscidians. The Folsom points are *never* associated with these elephants, which apparently became extinct before the large, Pleistocene bison. Folsom points are almost always associated with bison in the west. The only way Bowes could get the impression of elephants here would be through some faulty psychic connection, since Lindenmeier is a Folsom-period site. The list of faunal species recovered at Lindenmeier includes pronghorn antelope, wolf, coyote, swift fox, red fox, jackrabbit, and bison (*Bison antiquus*) (Wilmsen 1979:23). The list is notably devoid of elephants of any genus or species. Bowes's mention of elephants or mastodons could have come from no psychic vibration from the Lindenmeier artifacts. I would suggest that either he forgot the subtle distinction, or the book he

got his general information from did not provide that little detail, which, in any case, seriously calls into question the entire experiment.

The Lindenmeier experiment is typical of the rest of Jones's book. Any time Bowes or the others say anything that might possibly be construed as correct, the only possible explanation is psychic power. When Bowes identifies the source of some Mayan artifacts as the Vikings, it is clearly correct because both Vikings and Mayans were warlike, seagoing, and traders (Jones rationalizes this on page 181). When Mayan astronomical observatories are described as storage areas, it is suggested that they *might* have stored material there. When some very amorphous stones from the Yucatan Peninsula are located by Bowes in North Carolina, Florida, Texas, or South America, it's a hit (each successive guess comes closer to the actual source). When another of the psychics lists Lake Erie, Louisiana, Missouri, and Mexico for the same stones, it's a direct hit (pp. 231-232).

Even when they are clearly wrong, it proves psychic power, since the reading may not relate to the manufacturer of the artifact but, perhaps, to someone else who owned or touched the artifact at any point in time (p. 39), or it may have been contaminated by contact with some other artifact.

Perhaps the best tactic of all in the arsenal of psychic archaeologists is vagueness in the face of a friendly researcher. The vast majority of the data provided by the psychics is untestable personal information about the makers of the artifacts they analyzed. Even on specifics they are vague, though Jones is misleading on this point. When he states, "It would take a highly trained and experienced expert to differentiate a piece of primitive basketry of Bontoc or Mono origin" (39), the implication is that the psychics can do so. They don't.

When he describes Bowes's description of wrapped and boxed toy combs from Boston in the 1840s, he claims that it is "full of references to a mid-1800s Boston" (p. 26). While there are references to a body of water and lots of different kinds of people and buildings, there are *no* specific references to Boston or the 1800s. The only specific reference is to Eskimos. Finally, when detailing the Lindenmeier experiment he states: "No one, not even the most expert New World archaeologist, could look at a Folsom point and tell it came from Lindenmeier" (p. 77). I read into this the implication that the psychic can tell and did state that the material was from *Lindenmeier*. He, of course, never does.

The Jones experiments are not experiments at all—they are rationalized "proofs" of the existence of a highly questionable phenomenon. I do not believe that Jones is dishonest. If he were, he would not have supplied the full transcripts of the experiments in the appendices. Instead, Jones is clearly a researcher who has abandoned science,

reasoning, and rationality.

I, for one, am not going to replace my trowels with little Jeane Dixons. Instead, my research in FRAP will plod slowly along, underfunded and understaffed. We will advance one step at a time in our understanding of the prehistoric inhabitants of the Farmington Valley of New England, and, in essence, of all humanity. The human past is a fascinating enough field without wallowing in the mire of the miracles of lost continents, ancient astronauts, or psychic archaeology.

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On superstition

Such is the way of all superstition, whether in astrology, dreams, omens, divine judgments, or the like; wherein men, having a delight in such vanities, mark the events where they are fulfilled, but where they fail, though this happen much oftener, neglect and pass them by.

Francis Bacon, *Novum Organum I*

Beware of the "Truth Evaluator"

Philip J. Klass

"Scientific Truth Evaluator Shows . . . U.S. Air Force Officers Lied About UFOs" read the headline for the article in the August 15, 1978, issue of the *National Enquirer.* The article began: "Three U.S. Air Force officers who flatly denied that the government is investigating major UFO-encounters were not telling the truth, according to a stress analysis of their voices. 'There is a clear pattern (of deception) here that is common to all of them,' declared C. R. McQuiston, who is the co-inventor of the Psychological Stress Evaluator (PSE). 'They all had characteristics that lead me to believe there is indeed a cover-up.' "

The article continued: "McQuiston analyzed the answers the men gave to *Enquirer* questions in recorded interviews in connection with our June 27 article charging the government with a shocking on-going cover-up of UFO sightings. The PSE is so reliable that it has been used by more than 300 law enforcement agencies in the United States."

Later, when the same PSE expert analyzed a tape recording of Carl Higdon recounting his story of having been abducted by a flying saucer and carried off to its native planet before being returned to Earth, McQuiston concluded: "The PSE proves something extraordinary happened to Higdon. . . . He did have a strange experience," according to the October 24, 1978, issue of the *National Enquirer.* McQuiston admitted that "some parts of his statement show stress, but other parts show no stress at all, indicating he's telling the truth."

The October 24, 1978, issue of another tabloid, *Midnight/Globe,* reported that PSE tests had confirmed the story of Francie Steiger, an "internationally known psychic," that she is in daily contact with "her

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guardian angel, named Kihief." The analysis of Steiger's tape-recorded account had been performed by Forrest L. Erickson, who was identified as "one of the country's leading PSE examiners." When he analyzed a tape recording of Charles Hickson, one of two men from Pascagoula, Mississippi, who achieved instant fame in the fall of 1973 by claiming they had been abducted by a flying saucer and given a physical examination, Erickson concluded, "Without a doubt Hickson is telling the truth," according to an article in *UFO Report* magazine (November 1978).

When he made a PSE analysis of a tape recording of Travis Walton describing how he had been "zapped" by a UFO on the evening of November 5, 1975, Erickson concluded that Walton was telling the truth. But in analyzing subsequent portions of the account, in which Walton described regaining consciousness aboard a flying saucer and seeing strange-looking creatures, Erickson reported finding some evidence of heavy stress—indicating that Walton was not telling the truth.

This prompted Erickson to conclude: "We do believe that Travis Walton did see an object that could be classified as a UFO. We do believe that he was knocked unconscious. And we do believe that he struck someone in the spacecraft [UFO] and we do believe he was in the spacecraft. But everything that he relates, other than striking someone in the spacecraft and the events after he regained consciousness after leaving the spacecraft, appears to be very high stress and appears to be positive deception." In other words, Erickson accepts Walton's story that he was abducted and taken aboard a flying saucer, based on his PSE analysis, but concludes that Walton is not telling the truth about what he says occurred while he was aboard the UFO. Erickson does not speculate about why Walton would falsify this portion of his story.

McQuiston was asked to analyze tape recordings of Walton's story, and he concluded: "[Walton's] stress factors indicate to me there is little if any possibility of a hoax involvement in telling the story," according to an article by Jerome Clark, published in the October 1978 issue of *Fate* magazine. McQuiston acknowledged that he had "noticed several indications of what may well be embellishments of facts . . . but in no way do they interfere with my impression that he [Walton] really believes what he is saying."

Only four years earlier McQuiston had testified under oath during Congressional hearings that PSE could not possibly be used to evaluate the truthfulness of a person's statements without using rigorous structured test procedures like those employed in polygraph tests. The hearings were held on June 4-5, 1974, by the Government Operations Committee, to consider the federal government's use of "lie-detection" equipment and telephone monitoring devices and whether this use amounted to an

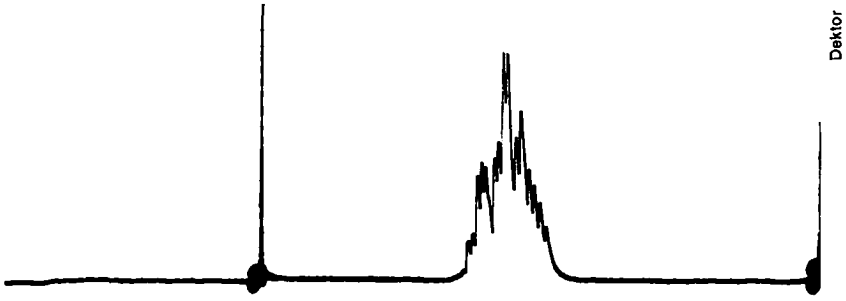
invasion of privacy. McQuiston, one of the inventors of the PSE technique, was then a senior vice-president of Dektor Counterintelligence & Security, Inc., the only company then producing PSE equipment.

The most succinct description of the principles involved in PSE, contained in the published record of the hearings, can be found in a one-page summary prepared by Dektor, portions of which are quoted below:

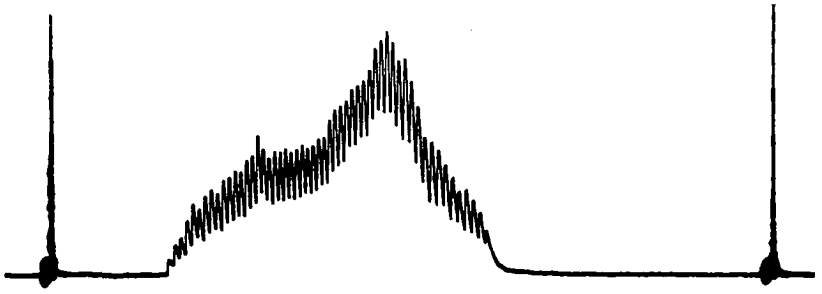
> When one speaks, the voice has two modulations—audible and inaudible. The audible portion is what we hear. The inaudible modulation comes from the involuntary areas (those not totally controlled by the brain or thought processes). Internal stress is reflected in the inaudible variations of the voice. These differences *cannot* be heard, *but* they *can* be detected and recorded by the PSE.
>
> The key to successful use of the PSE is the preparation of simple selected questions keyed to the individual and structured to reveal normal or truthful answers and answers that are false. Once the personal pattern has been established any evasive or false answers reveal stress; if a person is not telling the truth then analysis of his voice pattern will show it. The PSE has two great advantages over any similar instrumentation. First is its simplicity; it has few moving parts and is relatively easy to learn to operate. Second, the PSE does *not* have to be used at the time of the interview or interrogation. A tape recorder is used to make a permanent record of the interview. The tape is later fed into the PSE and the voice reactions recorded on the chart. Our examiners frequently make tape recordings for clients over the telephone, run the tape on the PSE and notify the client of the results of the test.

If the presence, or absence, of an inaudible "micro-tremor," as it is called, can be used to detect stress, as PSE proponents claim, it is still quite evident from the Dektor statement that PSE cannot properly be used as a "lie-detector" unless the subject is responding to "simple, selected questions . . . structured to reveal normal or truthful answers and answers that are false."

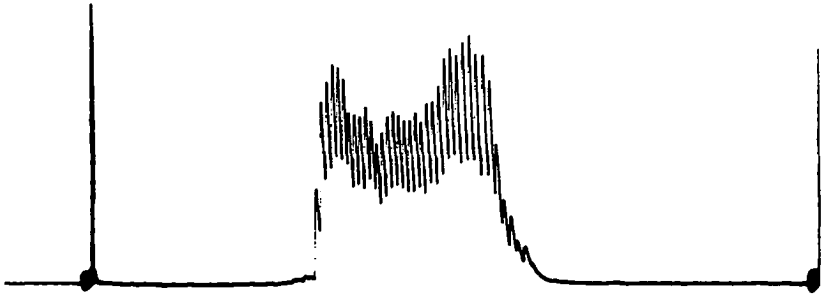
This also is fundamental to the use of the polygraph. The subject is asked simple, direct questions that he or she must be able to answer with an unequivocal "yes" or "no." the intended questions are discussed with the subject prior to the test to assure that there is no ambiguity and that the subject feels able to respond with an unqualified "yes" or "no." This also assures that there will be no stress generated by surprise questions. Typically the polygraph test includes "relevant" questions, such as, "Have you ever stolen money from your employer?" as well as "irrelevant" ones that should generate no stress, such as, "Do you have a driver's license?" Additionally, there will be control questions during the preliminary interview, when the subject is hooked up to the polygraph, to measure his response when he is known to be telling a falsehood. (For example, the subject may be asked to write down a number from zero to nine while the



VERY LITTLE STRESS



MODERATE STRESS



HARD STRESS

Charts showing stress measurements by PSE

examiner turns away. Then the subject will be told to answer "no" to the next ten questions, when the examiner asks: "Did you write down the number zero?" "Did you write down the number one?" etc. Unless the examiner can readily determine the correct number from the recorded chart of the subject's physiological responses, indicating that the subject does react to telling a falsehood, there is nothing to be gained by proceeding with the formal testing.)

McQuiston's testimony reveals that earlier he had been trained as a polygraph examiner at an army center, one of the best polygraph schools in the nation, and had worked in this field for more than a decade while in army intelligence. With this background, it is hardly surprising that McQuiston would recognize the need for using a well-structured test procedure for PSE, since both lie-detection techniques depend upon detecting *normal* stress in the subject. McQuiston emphasized these necessary constraints repeatedly during his testimony. For example: "PSE as a lie detector cannot be used without the knowledge of the individual because [lie] detection requires a very specific set of circumstances, which means a personal confrontation . . . test questions which must be reviewed with the individual prior to the test . . . You cannot conduct detection (of deception) surreptitiously. It is impossible with our equipment."

Later in the testimony McQuiston added: "There are many reasons that a person will show this stress level. First of all, his inherent level, his normal stress level . . . We have to know that as a base ground-truth before we begin our analyses. Certain elevations of stress level could be caused by many things. If we were to conduct an interview over the telephone, we do not control the distal end of that conversation. Any emotional involvement at the other end may show a stress level . . . That is why the controlled (test) structure is needed . . . That is why the controlled structure on the polygraph and PSE must be absolute. It must guarantee that the examiner is guided to the correct solution [i.e., reason] for the stress."

Yet four years later, after analyzing a tape recording of a telephone interview with USAF public affairs officers in the Pentagon—far from a structured test—McQuiston would be quoted as saying: "There is a clear pattern (of deception) here that is common to all of them. They all had characteristics that lead me to believe there is indeed a cover-up." And the *National Enquirer* would headline the article: "Scientific Truth Evaluator Shows . . . U.S. Air Force Officers Lied About UFOs."

McQuiston's statement in the tabloid was at such variance with his earlier sworn testimony before Congress that I wondered if he had been misquoted. On August 25, 1978, I wrote to McQuiston, care of Dektor, enclosing a copy of the *National Enquirer* article and asking if he had accurately been quoted. On August 31, I received a response from a Dektor

> . . . According to expert testimony before a Senate Judiciary subcommittee, when it comes to spotting the liars among us, the PSE is no more reliable than the toss of a coin. There is substantial evidence, in other words, that the PSE and other voice analyzers are utterly unreliable in differentiating between truth and deception . . . Consider . . . my own experience with the machine, I asked Edward Kupec, Dektor's marketing chief, to test me. To five related questions, I lied twice and told the truth three times. When I lied the machine acted as if I'd told the truth. And when I told the truth, the machine called me a liar.
>
> —John A. Jenkins, Washington journalist, in an investigative article on PSE in the *Washington Post* (Sept. 2, 1979)

official informing me that McQuiston was no longer associated with the company but that my letter had been forwarded to him at his residence in Florida. When six weeks had elapsed without a reply, I wrote directly to McQuiston, enclosing a copy of my original letter and asking if he had been correctly quoted in his more recent appraisal of the Travis Walton "UFO abduction," as reported in *Fate* magazine. I never received a reply from McQuiston.

The 790-page report, issued following the 1974 Congressional hearings, reveals that sharp controversy swirls around PSE and its intrinsic effectiveness. The Defense Department purchased five PSE machines, three of which were used to run controlled tests at Fordham University. These indicated that PSE was far less effective than the polygraph, even when used under similarly controlled situations. Another PSE was evaluated by the Air Force Office of Special Investigations, which concluded that it "was not useful." A fifth was tested by the National Security Agency, which concluded that it was "insufficiently reliable." The Fordham test procedures were challenged at the hearings by PSE proponents who introduced supportive test data. For example, limited tests conducted at Washington and Lee University indicated that PSE showed promise. Tests conducted at the University of Utah suggested that "a certain level of stress must be reached in the individual before changes in the voice occur; but once that level of stress is exceeded, differential changes in the voice can be used to determine deceptiveness." But this is a qualified endorsement of PSE principles when compared with claims advanced by some of its proponents.

The American Polygraph Association is critical of PSE as a "stand-alone" lie-detector, arguing that measurement of a single physiological response is not sufficient. (The polygraph measures and records the subject's breathing pattern, heart-beat, and perspiration.) When PSE is

used as an additional sensor of physiological stress effects, APA views it as a possible aid.

The APA is also critical of the very brief training given by Dektor to persons who purchase their PSE equipment. Originally the training was only three days, but Dektor recently has increased it to five days. The basic training period for polygraph examiners at the army's school at Fort McClellan, Alabama (previously located at Fort Gordon, Georgia), is *14 weeks*. This is followed by a six-month internship working under experienced polygraph examiners. Then, several years later, the graduate returns to Fort McClellan for a three-week advanced course. This thorough training explains why the army school is considered one of the best in the nation. Private polygraph training schools typically have a six-week educational period followed by a 6- to 12-month internship period. If Dektor were to limit the sale of PSE equipment to persons already trained in polygraph, its very brief training period would not come under such sharp criticism. But anyone with $5,000 can buy a machine and become a PSE examiner in less than a week's time.

Thus there are many who can claim to be PSE experts. One PSE expert, quoted in an article on lie-detection in the September 1977 issue of *Oui* magazine, "concluded that Lee Harvey Oswald was innocent of killing President Kennedy on the basis of PSE analysis of Oswald's recorded statements after he was arrested. Other [PSE] investigators have discovered that Edward Kennedy's version of Chappaquidick does hold water."

When you read that some fantastic tale has been substantiated by PSE on the basis of analysis of a tape recording of a casual conversation or interview, it is well to recall still another statement that McQuiston made under oath at the 1974 hearings: "If the purpose is 'lie detection,' the necessary personal pretest interview and specific set of control questions require that the examination be overtly administered with the subject's full knowledge and consent . . . *If there were stress, the officer* [PSE analyst] *could not determine that the subject was lying without using those control elements for the analytical process by which the stress is equated to attempted deception.*" (Emphasis added.)

Later McQuiston added: "When stress indicators are instrumented under carefully controlled conditions of an appropriately formulated test, and therein analyzed to determine causality, an experienced and perceptive inquirer can distinguish the stress caused by an attempted deception from stress resulting from other possible causes."

Remember this critical caveat when you read that you can buy a PSE built into a wristwatch-size case. According to an article in the April 11, 1979, *Washington Post*, Communication Control Systems Inc., using the

modern miracle of microelectronics, hopes to offer such a "wristwatch PSE" for sale for only $29.95. Instead of producing charts for analysis, there will be eight tiny lights. "Should most of these diodes light up when a person replies to a question, that will mean the subject is relaxed," the *Post* article said. "On the other hand, should only one or two diodes light up, it would suggest that the speaker is under stress (and possibly lying)," the article stated.

The article continued: "The prospect of such an eerie eavesdropping watch is ominous. It could signal the end of trust in America. Clearly, it would mean widespread invasion of privacy—1984 and Big Brother, five years early."

Will the Federal Trade Commission require that excerpts from McQuiston's sworn testimony in 1974 be printed on the box that encloses the wristwatch PSE? I doubt it. When millions of persons can become "PSE experts" for only $29.95, perhaps even McQuiston himself will better recall his repeated, strongly emphasized caveats made in 1974. •

Evolution and the round earth

Pedagogic "proofs" of the past occurrence of evolution are of the same kind and unfortunately the same intellectual stature as those "proofs" of the roundness of the earth which we learnt in our earliest schooldays. It is not upon these so-called "proofs," however, that the acceptance of such a hypothesis depends. It is rather that the hypothesis of evolution pervades, underlies and makes sense of the whole of biological science in much the same way as the idea of the roundness of the earth permeates the whole of geodesy, chronology, navigation and cosmology. The evolutionary hypothesis is part of the very fabric of the way we think in biology. Only the hypothesis of evolution makes sense of the obvious inter-relationships between organisms, the phenomena of heredity and the patterns of development. For a biologist the alternative to thinking in evolutionary terms is not to think at all.*

—P.B. Medawar and J.S. Medawar, *The Life Science*, Harper & Row, 1977

**The word "hypothesis" in this context is used in its correct and technical sense: it is a vulgarism to suppose that the word has a pejorative flavour and that in describing as a hypothesis what is usually called the "theory" of evolution we are in some way depreciating it.*

Book Reviews

Observing UFOs. By Richard F. Haines. Nelson-Hall, Chicago, 1980. 300 pp.

Reviewed by Armando Simon

If there is one field that should have had first dealings with UFOs it is psychology. And if there is one area in psychology that is of paramount relevance to the subject in question, it is that of perception.

Richard Haines is a perceptual psychologist. Because of his training, the book he has written, *Observing UFOs*, is one that at long last fills in the glaring gap that has existed in the field of flying saucers for over 30 years.

The purpose of the handbook is fourfold. First, it attempts to upgrade the quality of training by field investigators. As such, the book supplements the already existing training manuals of private organizations.

> Most field investigators need more specialized training. They need to be shown the critical importance of not biasing the witness, of establishing a clear and concise terminology, of gaining the confidence and trust of the witness, of obtaining enough verbal and non-verbal information from the witness and others who are involved, but not so much that the privacy of the witness is invaded, and of preparing a final written report that is fully documented and understandable to others. (p. 45)

Second, Haines suggests that it is a mistake to think UFOs cannot be tested with the scientific method. The problem with UFOs is that it is much harder to do so. Therefore, if all parties concerned keep in mind various principles and limitations, it is definitely possible to obtain *quantifiable* data that is amenable to statistical analysis and that may just reveal the "core" of the phenomenon. We need not wait for breakthroughs in the various scientific disciplines to analyze UFO reports. Third, since we are all potential observers of future UFOs, we should maximize our opportunity by making as accurate an observation as possible. For example, in a nighttime sighting, writing down how long we were away from bright lights in order to ascertain the UFO's brightness (several tables containing potentially useful

Armando Simon is a psychologist.

information are in the book) or the names and brightness of stars against which to match illuminance and trajectory. Last, Haines presents the reader with established principles of perception that help to determine what is and is not possible. These principles fall into the areas of memory, honesty, and perception itself. As the Condon Report does, Haines reviews basic principles of visual perception that are generally unknown among both the public and UFOlogists (e.g., autokinesis, the Bezold-Brücke phenomenon, Charpentier's illusion, the irradiation phenomenon, time passage estimation, and so on). Several instruments are presented as potentially useful and purchasable. "Perhaps the main point is that, *through the proper application of research data, one may reconstruct an approximate idea about the true nature of the visual phenomenon . . .* "(p. 248, italics in original).

There are two minor shortcomings: (1) Like most technical handbooks, it makes dry reading. Haines purposely omits cases. After a steady dosage of sensational cases elsewhere, this is quite a switch. (2) Haines from the very beginning states that he will deal only with visual perception, not with other psychological topics like hypnotic regression. That is legitimate. However, the topic of "mental set" (how anticipation and previous exposure to stimuli affect perception) does belong in the book, and I was sorry it was not included. *Observing UFOs* should be read in conjunction with another book, which Haines edited, *UFO Phenomena and the Behavioral Scientist.* The latter, the work of eleven social scientists, deals with many of the other psychological facets of UFO observation besides perception.

It is revealed throughout the book that, although Haines is open-minded to the possibility that UFOs may be something highly unusual, his feet are planted firmly on the ground, and he refuses to confuse speculation and wishful thinking with facts. •

UFO Sightings, Landings and Abductions. By Yurko Bondarchuk. Methuen Publications, Toronto, 1979. 208 pp.

Reviewed by John Kenny

If the blues have got you down, I highly recommend you read *UFO Sightings.* Mr. Bondarchuk has produced a work of unparalleled amusement and drollery. Unfortunately, one suspects this was not his intent.

The "documented evidence" is organized into 13 chapters—"The Photographic Evidence," "UFO Landings and Physical Traces," and "UFO Abductions," to name a few. The book is well illustrated with pictures of unusual phenomena and personalities. There are also cute little sketches by the author of ominous looking beings with bulbous heads and mysterious craft zapping people and hovering over nuclear installations.

This book does have its good points. First of all it examines the phenomena from a Canadian point of view, bringing together most of the major reports from across the nation. Second, it points out the need for the government to relax some

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of the security restrictions on its UFO files. This secretiveness merely fuels the controversy and, as Mr. Bondarchuk demonstrates by his own writing, in the absence of hard data the UFO buffs invent their own.

The "documented evidence" includes references to hyperoptic travel, parallel universes, and visitors from not only other star stystems but distant galaxies. Aside from these excursions into fantasy, the book is replete with self-contradictions, alternately stating that the UFOs are concerned over our deteriorating environment and accusing them of dumping their waste in our lakes; blaming the rising cost of electricity on UFOs' tapping high tension lines and postulating the great '65 blackout was caused by a beneficent UFO supplying a huge surge of power to the Adam Beck generator outputs. The book moves from the ridiculous to the ludicrous with gems such as UFO dwarfs stealing potted flowers from a little old lady. Even well-explained sightings, such as President Carter's spotting of Venus and the flap over the Japanese shrimp-boats off the coast of New Zealand, are not safe from being once more produced as proof positive of an alien presence.

Throughout there is an air of anthropomorphism. UFO occupants are described as being humanoid in form and come in male and female. Those that are not humanoid are described as being subservient to the humanoids. The items of concern to the UFOs also reflect the current concerns of society at large: the environment, the possibility of a nuclear holocaust, and the energy crisis.

Bondarchuk ends with his personal prognosis for the future of UFO phenomena: either we will see less, things will stay the same, or we will see more. Now who can argue with logic like that? •

Pyramids and the Second Reality. By Bill Schul and Ed Pettit. Fawcett Books, New York, 1979

Reviewed by James Randi

There are literally hundreds of books offered today that can serve as horrible examples of misinformation, but this volume is the Rosemary's Baby of them all. I can recommend it as a classic.

To list all the errors, examples of naiveté, and outright misrepresentations would take a book in itself. Leafing through it I came upon mention of two scientists, "D. Bohn" and "David Bohm." They are referred to as different persons in the text and listed separately in the index. But they are the same person. Jack Sarfatti (formerly "Sarfatt") is called repeatedly by the name "Safritti"—but correctly as one person.

The old chestnut about some ancient pharaoh's body tissues being revitalized "by scientists" is repeated to prove the preservative power of the pyramids of Egypt, despite the fact that no ruler was ever found in such a structure, and the claim about viable cells is pure nonsense.

Of course, Uri Geller is dragged in proudly, and reference is made to experiments with him that never took place, such as the "dematerialization" that was done under "carefully controlled conditions"—actually referring to another set of silly tests done with another psychic claimant. The name of Isaac Asimov is invoked, and quotations are given from his works, taken to prove totally unsupportable ideas concerning "pyramid power." As we might suspect, the now-

embarrassing story of the Berlitz pyramid that scholars discovered beneath the Atlantic off Bimini is swallowed whole and used to bolster the case.

Perhaps the most convincing event in the book concerns a young girl who was cured of bedwetting when a pyramid was placed beneath her bed. If I saw Schul and Pettit placing the dumb thing under *my* bed, I don't know *what* I'd do. •

Some Recent Books

Listing here does not preclude a detailed review in a future issue.

Culver, Roger B., and Philip A. Ianna. *The Gemini Syndrome: Star Wars of the Oldest Kind.* Tucson, Ariz.: Pachart Publishing House (P.O. Box 35549, Tucson, AZ 85740), 1980, 216pp., $11.95. Fair-minded scientific appraisal of the claims and methods of astrology by two astronomers from Colorado State University and the University of Virginia. Concludes with a formal challenge to any astrologer to submit to one of ten tests of astrological ability.

Marks, David, and Richard Kammann. *The Psychology of the Psychic.* Buffalo, N.Y. : Prometheus, 1980, $15.95. With a foreword by Martin Gardner. Psychologists examine the claims of supposed "serious" researchers like Targ and Puthoff and the conjuring methods of "mentalists" like Kreskin and Geller. Also contains chapters on pathological science, coincidence, self-perpetuating beliefs, and the art of doubt.

Vogt, Evon Z., and Ray Hyman. *Water Witching U.S.A.* 2nd ed. Chicago: University of Chicago Press, 1979, 260 pp., $5.95, paper. Welcome new edition of classic work on dowsing by Harvard anthropologist and University of Oregon psychologist. Also deals with related topics of "mind reading" horses and people, turning tables, magic pendulums, and Ouija boards. Such devices and supposed abilities depend, they find, upon involuntary motor action of subjects. A new "postscript" chapter focuses on the rise of urban dowsing, attempts to divine the location of underground and lost objects, diagnose diseased organs, predict future events, and analyze character.

—*K.F.*

Articles of Note

Following is a sampling of some recent articles that critique or report on paranormal or fringe-science claims.

Anderson, Duncan M. "The #1 Skeptic and His Debunking Brigade." *Science Digest* (Special Edition), Spring 1980, pp. 80-83, 118. Profile of Paul Kurtz and CSICOP.

Broad, William J. "Paranormal Powers Are So Much Hocus-Pocus." *Science*, 207 (Jan. 25, 1980), p. 389. Brief news article on James Randi's current investigations.

Browne, Malcolm W. "Arguing the Existence of ESP." *New York Times,* Jan. 29, 1980, p. C1. Mostly balanced report on scientific attitudes toward ESP.

Cole, John R. "Cult Archaeology and Unscientific Method and Theory." In *Advances in Archaeological Method and Theory,* vol. 3, ed. M. Schifler, Academic Press, 1980. Lengthy survey of characteristics of cult-archaeology approaches that share "the common denominators of sensationalism, misuse of logic and evidence, misunderstanding of scientific method, and internal contradictions in their arguments," as well as other traits of pseudoscience. Includes case studies of ancient astronauts, ancient inscriptions, early-New-World-man cultism, and psychic archaeology.

"The Conversion of Kübler-Ross." *Time,* Nov. 12, 1979, p. 81. Discussion of Elisabeth Kübler-Ross's links to Jay Barham's Church of the Facet of Divinity and its sessions of sexual intercourse with people's "entities," in at least one case said to be Barham.

"Dean Justifies Psychic Research." *Science News,* Nov. 24, 1979, pp. 358-59. Nonevaluative news report on Princeton engineering dean Robert G. Jahn's entry into psychic research.

Feder, Kenneth L. "Foolsgold of the Gods." *Humanist,* Jan.-Feb. 1980, pp. 20-23. Anthropologist supplies data from *Chariots of the Gods?* indicating that von Däniken's arguments are strongly motivated by racism: Europeans largely escape von Däniken's our-ancestors-were-dummies argument; 96 percent of the examples he uses to "prove" ancient peoples were too unsophisticated to accomplish technological tasks without outside help are drawn from Africa, Asia, and North and South America.

Fishman, Paul. "Psychic's Tricks Really Magic." *Twin Cities* (Minn.) *Reader,* vol. 4, no. 42, Nov. 7-13, 1979. Report by a professional magician outlining how Uri Geller bends spoons and reads minds by conjuring trickery. Written after a performance by Geller at a local university ("What you see here tonight is real, not a trick") that left the audience wowed.

Helmers, Carl. "Is Pseudoscience Done by Computer Pseudo-Computer-Science?" *BYTE,* November 1979, pp. 6-9. Editorial criticizing the flood of interest by computer fans in birthdate-based biorhythms and the notion that "if it is represented in a programmed calculation, it must be true."

Jacobs, Paul. "Iridology Fails in Tests by UC San Diego Experts." *Los Angeles Times,* Oct. 9, 1979, p. 1. Report by medical writer gives additional information on the controlled tests by Simon, Worthen, and Mitas (see below) of claims of iridology (the idea that the eye's iris is a map of body disorders). The experiment, termed by one eye-specialist "the first genuine piece of scientific research on the subject," found no support for the claims of iridology.

Kerr, Richard A. "Venus and Science's Fringe." *Science,* 207 (Jan. 18, 1980), p 293. Brief report on how latest analyses of data from Pioneer Venus spacecraft further undermine Velikovsky theories.

Kimelman, Donald. "A Strict Diet for the Soul—and a Death in West Philadelphia." *The Philadelphia Inquirer,* Oct. 14, 1979, p. 1. Lengthy report on the life of 36-year-old David Blume, who starved himself to death on a near no-food diet "for the soul" under the tutelage of the Temple Beautiful.

Leff, David N. "Four Wondrous Weeks of Science and Medicine in the Amazing, Incredible Supermarket Press." *Newsletter, National Association of Science Writers,* Jan. 1980, pp. 3-7. Entertaining review of the ways the supermarket

tabloids pitch titillation and new hope in the guise of science. What factual reporting there is "is cocooned in adjectives and interlarded with so much modern mythology that its information titer is diluted to the pico-range." Includes a lexicon of checkout-counter-press hyperbole.

Morris, Scot. "Interview with James Randi." *Omni,* April 1980. A no-punches-pulled interview with one of the best-informed and most outspoken critics of paranormal claims. Touches on Geller, the SRI experiments, dowsing, "psychic" photography—the whole litany of psychic claims. Randi's honesty and clear perception of sham and self-delusion shine forth throughout.

Myers, Dan. "It's Not Written in the Stars." *Honolulu Advertiser,* Jan. 2, 1980. Analyzes predictions made one year earlier for 1979 by "10 Leading Psychics" in *National Enquirer.* "Somehow the 10 psychics missed the Middle East accord, Iran, Three Mile Island, Skylab, and all the rest of what did happen in 1979, achieving a batting average of approximately zero."

Rosnow, Ralph L., and Allan J. Kimmel. "Lives of a Rumor." *Psychology Today,* June 1979, pp. 88-92. Two specialists in hearsay reflect on the psychology of rumors and why some reappear again and again.

Simon, Allie, David M. Worthen, MD, and John A. Mitas II. "An Evaluation of Iridology." *Journal of the American Medical Association* 242:1385-1389, Sept. 28, 1979. Results of a controlled study by researchers at the University of California Medical Center and the VA Medical Center, both in San Diego, This first scientific study of the claims of iridology (that the iris of the eye reveals body ailments) found that iridology had no clinical or statistically significant ability to detect the presence of kidney disease. The likelihood of correct detection was statistically no better than chance.

Walton, Susan. "Holistic Medicine." *Science News,* Dec. 15, 1979, pp. 410-412. Well-balanced analysis of the mixed bag of treatments and procedures lumped together as holistic medicine. Attempts to make necessary distinctions between those aspects that involve medical fads of questionable efficacy or occultic derivation and those that involve merely a return to a conservative philosophy of patient care that emphasizes the whole person rather than the disease.

Wilcox, Laird M. "Astrology, Mysticism and the Occult: A Critical Bibliography." Kansas City, Mo.: Editorial Research Service (P.O. Box 1832, Kansas City, Mo. 64141), 1980, 29 pp., $4.95. Bibliography, briefly annotated, of 420 articles and books that present critical and factual information and analysis of occult and pseudoscience claims. Most are from past five years. Includes an introduction and brief bibliographies of books on logic and rational thinking and on propaganda technique. Updated and much expanded version of first (1978) edition.

Wood, L.A., D.W. Krider, and K.D. Fezer. "Emergency Room Data on 700 Accidents Do Not Support Biorhythm Theory." *Journal of Safety Research* (National Safety Council), vol. 11, no. 4 (Winter 1979): 172-175. Study of hospital records of patients suffering self-inflicted accidental injuries shows no correlation between day of accident and "dangerous critical days" of birthdate-biorythm theory.

—*K.F.*

Follow-up

The following two articles continue the discussion that began in the Winter 1979-80 SKEPTICAL INQUIRER concerning the so-called Mars effect on sports champions—the claim by Michel and Francoise Gauquelin that especially outstanding athletes tend to be born preferentially when Mars is in two certain sectors of the sky. The four-part report in that issue began with the results of the first study of U.S. sports champions, conducted by Paul Kurtz, Marvin Zelen, and George Abell. They concluded that their study showed no evidence of the "Mars effect." Their report was followed by an article by the Gauquelins offering a considerably different interpretation of the same data. The section concluded with "Response to the Gauquelins" by Kurtz, Zelen, and Abell. In the following article, Michel Gauquelin offers his comments on the Kurtz group's response. Following it is a rejoinder from Kurtz et al. The Editor

The "Mars Effect": A Response from M. Gauquelin

Two years ago, Paul Kurtz, Marvin Zelen, and George Abell stated: "A member of the CSICOP selectively inspected the Gauquelins' archives and was impressed by the meticulous care with which the data had been collected" (*Humanist*, Nov./Dec. 1977). The fact is that we published in our scientific reports all the data, quoted all the reference books used, and detailed our criteria for objective selections when it was needed; and we always sent to the members of the CSICOP all the documents and information they asked for.

But now that Professors Kurtz, Zelen, and Abell have gathered a sample of U.S. athletes and the Mars effect has shown up with the most outstanding of them, exactly as one would predict from our prior results, these authors try to discredit us (SKEPTICAL INQUIRER, Winter 1979-80). They suggest that we cheated when we gathered the Parisian data for the Zelen test, when we demonstrated that the most famous U.S. athletes display the Mars effect, when we recently replicated this effect on a new sample of European sports champions, and even when we carried out our first experiment that revealed the Mars effect, twenty years ago. If so, how do Kurtz, Zelen, and Abell explain that, in 1968, the Belgian Comité Para also

replicated the Mars effect on an entirely different group of sports champions. Would they suggest that the more than skeptical scientists of the Belgian Comité Para also cheated in obtaining a Mars effect?

The various accusations published by Kurtz, Zelen, and Abell in their "Response to the Gauquelins" demand a reply.

I. Concerning the outcome of their experiment on U.S. athletes:

- Kurtz, Zelen, and Abell seem surprised that we question the content of their sample. Had they asked our opinion about their choice of reference books *before* the experiment, we would not have had to question their compilation. But they did not. We certainly would have agreed with the choice of the *Lincoln Library of Sports Champions,* but we certainly would *not* have agreed with the use of *Who's Who in Football* and *Who's Who in Basketball* without making any prior selection among the thousands of players included in these books.
- Kurtz, Zelen, and Abell do not explain why their first selection, which contains almost the totality of outstanding athletes from the entire sample, gives a significantly stronger Mars effect than their third selection, which contains very few renowned athletes. Whatever the subsample of the data selected (by Kurtz, Zelen, and Abell or by us) the percentage of Mars in "key sectors" is always higher for the more famous athletes than for the others. It is extremely difficult to invoke pure chance as an explanation for this.
- Kurtz, Zelen, and Abell refer to the *Hammond Almanac* and claim not to have found any significant Mars effect with the top athletes listed in it. In reality, they have picked out some of the outstanding athletes listed but omitted those for which the Mars effect is strongest. In the *Hammond Almanac,* pages 911 to 999 are devoted to sports; the *first list* published contains the names of the Olympic champions—track-and-field and swimming—who received a gold medal in individual contests. We found there twenty athletes that we had previously obtained from the *World Almanac and Book of Facts*, and for whom the Mars effect goes up to 35 percent! Other "forgotten" items: page 966 lists pro golf's leading money-winner per year, among whom Billy Casper and Jack Nicklaus belong to Kurtz, Zelen, and Abell's data; page 969 lists the U.S. Tennis Association National Champion in singles (one per year), among whom D. Budge, H. Wills, A. Gibson, J. Kramer, A. Ashe belong to Kurtz, Zelen, Abell's data; page 977 lists the NASCAR champions of stock car racing, among whom R. Petty and C. Yarborough belong to the same sample; etc. Consequently, an objective scanning of the sports champions listed in the *Hammond Almanac* adds 29 names to the 53 cases given by Kurtz, Zelen, and Abell. Among these 82 star U.S. sportsmen, 19 were born with Mars in "key" sectors, i.e., 23.2 percent, a significantly higher percentage than in the whole sample (13.5 percent). This percentage is remarkably similar to, but slightly higher than, the Mars effect for 88 U.S. star sportsmen we found in the *World Almanac and Book of Facts,* described in the SKEPTICAL INQUIRER (Winter 1979-80 issue): 19 with Mars in "key" sectors, i.e., 21.6 percent.
- *The World Almanac and Book of Facts* was the only reference book that we could find in Paris at the American Library for obtaining information on outstanding U.S. athletes. Now we are staying in San Diego, and we have found at the public library here several other books that all confirm that the top U.S. athletes display the Mars effect. Let us give only two examples: (1) *The Reader's Digest 1978 Almanac* lists on page 773 the major U.S. sports awards. The most

prestigious one, quoted first, is the "Athlete of the Year" chosen by the Associated Press. Since 1934, only one male and one female U.S. champion received this award per year. Out of this list, we find in Kurtz, Zelen, and Abell's data: Budge (awarded twice), G. Davis, Mays, Cassady, T. Williams, Maris, Schollander, Spitz, Ali (Clay), Wills, Gibson (awarded twice). Five of these 11 athletes were born with Mars in key sectors, i.e. 45.5 percent, while the percentage in Kurtz-Zelen-Abell's whole sample is only 13.5 percent. (2) The book *Superstars*, by Frank Litsky (1975), lists 280 athletes, with a majority of U.S. champions. "Here are the stories of the Superstars of the Superstars," says the author. Thirty-two of them are in Kurtz-Zelen-Abell's sample: Aaron, Ali (Clay), Ashe, Beard, Blanchard, Budge, Button, Casper, Chadwick, Counsilman, Cronin, Csonka, Dillard, Frazier, A. Groza, Justice, Lee, Luisetti, Mays, Nicklaus, Petty, Ryun, Sayers, Schollander, Spitz, Starr, Walton, Warmerdam, White, T. Williams, Wills, and Wynn. Ten out of these 32 superstars were born with Mars in key sectors, i.e., 31.3 percent, instead of 13.5 percent in the total U.S. sample. Frank Litsky is a renowned sports specialist. Could one claim that he cheated in order to help us demonstrate a Mars effect, when he made his superstar selection in 1975?

- For All-Star and All-Pro players, we must still question Kurtz-Zelen-Abell's selection criteria, which not only fluctuate from their first screening to others but are also undefined: if we take all selection of all leagues, there is no end to the selections (more interesting would have been the results for the players who were elected several times "unanimous All-American"). There may even be a suggestion of bias in the sampling here. In the *Who's Who in Football,* for instance, one case selected by Paul Kurtz as All-Pro was not mentioned as All-Pro in the book; Paul Kurtz explained in a letter that this player "was elected after the volume was printed." For another case erroneously taken as All-Pro, Paul Kurtz explained that this athlete "played in the Pro Bowl, which is All-Pro," which is obviously wrong: playing in a Pro Bowl is not the same thing as being elected All-Pro. Such a fluctuating procedure for selection of subjects would allow all kinds of conclusions and causes one to wonder at the true rationale for inclusion or deletion of subjects.
- Kurtz, Zelen, and Abell also write: "Given the fact that the United States has four times the population of France, one wonders if France produces more distinguished athletes than does the United States." The authors neglect to consider that in France: (1) there is no Privacy Act, and (2) the hour of birth has been recorded since 1793 on all birth records. In the United States, 60 percent of the states refuse to give the information; moreover, the hour of birth was not recorded until the first years of the twentieth century at best, and not even at present at the worst. Consequently, for the United States, we are deprived of information about all of the most outstanding players of the past, and many great contemporary players could not be included.
- In their "Response to the Gauquelins" Kurtz, Zelen, and Abell use and abuse the statistical "95 percent confidence limit," which appears in several tables of their article in such contradictory forms that we are wondering if this is due to typing errors or to an oversight from the authors of the paper.

II. But the worst part of Kurtz, Zelen, and Abell's "Response to the Gauquelins" certainly is their "Note added in proof" devoted to my recent replication of the Mars effect on a sample of 432 new European sports champions. I cannot accept the way they tried to escape this newest replication by discrediting

my sample. Let us tell the story: After the very encouraging results of the experiment with the star U.S. sportsmen, I wrote on November 10, 1978, to Kurtz, Zelen, and Abell that we were considering the possibility of making a new experiment on sports champions in Europe under the Committee's control. We received no answer to this letter. During a trip to the United States in March 1979, we visited P. Kurtz in Buffalo, M. Zelen in Boston, and G. Abell in Los Angeles. We repeated our proposal to participate in a new experiment, offering to give them the necessary directories we had brought with us. We suggested that they *themselves* could select in these directories the names of the most famous athletes listed. But they did not seem to desire to undertake this experiment. Therefore, we decided to carry it out ourselves. In September 1979, I sent to several members of the CSICOP a copy of my report on this experiment. This report gives, in detail, all the reference books I used and contains the names, births, and Mars data of the 432 famous new athletes I gathered. I also sent, by air mail, to Paul Kurtz in October 1979—at his own request—a copy of the huge *Dictionnaire des Sports* (Denoel, 1973), the main directory I used for gathering my new sample. But in their impatience to discount immediately this new replication of the Mars effect, Kurtz, Zelen, and Abell made a rather unfair use of the documents I sent to them. Let us see:

First, Kurtz, Zelen, and Abell note that I had difficulty obtaining data in Germany and Spain (suggesting apparently that there is a Mars effect in these countries only because I concealed some answers). But they forgot to say that I obtained almost all the data I requested in Italy, Belgium, the Netherlands, and Scotland, and that the Mars effect in these countries is stronger than in Germany (page 14 of my report).

Second, they are surprised by the fact that I did not exclude the athletes born after 1950 in my sample. But they forgot to add that I explicitly pointed out that the Mars effect disappears for the most recent births of my sample (page 18 of my report) and I hypothesized that this disappearance could be due to induced and medically altered delivery procedures.

Third, I clearly indicated in my report that *I did not exclude* any athletes from the directories I used for Italian, German, Belgian, Dutch, Scottish, Spanish, and Luxemburgian data. This group of European famous athletes—excluding those in France—has 182 births. Mars is in "key sectors" in 49 cases, i.e., 26.9 percent. That is very significant (page 14 of my report).

Fourth, it is only for the French part of my sample that I had to establish a selection, because, by an understandable chauvinism, the author of the French *Dictionnaire des Sports* had listed names of French sportsmen for nearly half of his book, which is clearly not the size of their importance in the international sports contests. In October 1979, I sent to Paul Kurtz—despite the fact that he had not asked for them—copies of the documents, allowing him to perform an objective selection of the French players who were selected for the French team for international games. He apparently neglected these documents. At any rate, the fact is that, among the 250 new French athletes gathered, 57 display the Mars effect, i.e., 22.8 percent. That is significant but less strong than for the other 182 Europeans where no selection had been necessary (page 14 of my report).

So when Kurtz, Zelen, and Abell dare to write that I excluded 423 other "famous" French sports champions from the sample, it is most aggravating. These so-called famous 423, mostly born between 1940 and 1950, are not famous at all;

otherwise the density of the French famous champions would be incredibly high compared with other European countries. But let us accept for a moment the reasoning of Kurtz, Zelen, and Abell and add the 432 "great" to the 423 "lesser" athletes, without any selection. What are the results? Kurtz, Zelen, and Abell do not describe them. But my report (page 21) gives the answer: "If we add the famous and the nonfamous champions, the number of subjects is 855; the number of Mars in key sectors is 177—that is 20.7 percent, a still significant figure, but its significance comes entirely from the famous athletes."

In conclusion, let me say that the series of articles and responses between the various parties to this discussion has been in fact agreeable to us. We have always been interested in arguments that attempt to disprove our surprising results. However, we must continue to assert that no counterevidence has yet been presented in this series or elsewhere that lead inescapably to the conclusion that our result is discredited. We can only wish that any continued discussion might be directed to the effect itself and the statistical or physical reasons for its appearance, rather than suggestions being made that its appearance is due to some bias in our sampling procedures, which is an accusation unwarranted and indefensible.

Michel Gauquelin
San Diego, Calif.

The Contradictions in Gauquelin's Research: Rejoinder by Kurtz, Zelen, and Abell

We thank Michel Gauquelin for his comments. While we understand his concern over our reservations about his response to our two papers in this journal (Winter 1979-80), we believe that he has misunderstood our main points. We regret that he has not replied to our principal criticisms but, instead, has chosen to raise a number of side issues that tend to obfuscate the key points of our critique.

I

To review the basic issues as we see them: In a meeting between Michel Gauquelin and Kurtz, Zelen, and Abell in July 1977, *before* we began our research, Gauquelin agreed upon the use of the *Lincoln Library of Sports Champions, Who's Who in Basketball,* and *Who's Who in Football,* although he now objects to the use of the latter two volumes. We later added *Who's Who in Track and Field* and *Who's Who in Boxing,* with his agreement. As a result of our careful inquiries, we obtained data for 408 U.S. sports champions. Mars was present in the "key sectors" only 13.5 percent of the time. Since chance would be 17.2 percent, and since Gauquelin predicts 22 percent for sports champions, we concluded that our survey of famous U.S. sports champions did *not* indicate the presence of a "Mars effect."

Gauquelin replied (*SI,* op. cit., pp. 31-34) that our sample did not include enough "all-time great" champions. Although all of the champions selected were listed in *Who's Whos* or the *Lincoln Library,* he contended that the sportsmen were not of "top rank." He then selected, post hoc, 88 sports champions (including 20 Olympic champions) who, he said, had achieved this rank. Gauquelin maintains

that this group of 88 displayed the "Mars effect."

In our response to Gauquelin (*SI*, op. cit., pp. 44-63), we questioned the basis of his selection—which, in any case, was too small a sample to conclude anything. However, we stated that if he *were* to make such a selection, there were other names that would have to be included in a list of "most outstanding" athletes:

53 additional names from the *World Almanac* and *Hammond Almanac* that he had omitted (see Table 8, op.cit., pp. 51-52). (In reply to Gauquelin's point above, we were aware that there were other names in the *Hammond* and *World* almanacs, but they were already listed by Gauquelin in his group of 88; the 53 additional names were to be added to his list.)

6 Olympic champions that he inadvertently omitted from his selection (3 won team gold medals).

65 additional All-Star and All-Pro team members (avoiding overlaps). (See Table 9, op.cit., p. 53.)

Note: In his new study of 432 European sports champions, Gauquelin included soccer players only if they had made the French national team at least once. This is apparently equivalent to the All-Star, All-Pro, or Pro-Bowl teams for U.S. basketball and football players. All-Pro and Pro-Bowl players have achieved levels of distinction similar to that of All-Star. Incidentally, we used only All-Stars cited in the reference books. Although one player, Daniel Abramowicz, was later elected to an equivalent All-Star team, we did not include him in our final list because Gauquelin objected to introducing any names that were not included in these volumes.

Therefore, by employing criteria not unlike those later used by Gauquelin in his European study of 432, we expanded his list of 88 "outstanding" athletes to 181. These 181 star champions showed an incidence of Mars of only 16.6 percent—or chance. Thus, even if one includes only the "most outstanding" athletes—we submit that all of the 408 sports champions in the U.S. study had achieved high merit—the "Mars effect" still does not appear.

We took issue with Gauquelin's procedure of referring to almanacs and directories after the fact and then selecting small lists of the "most outstanding" who display the "Mars effect." He repeats the same procedure in his response above by selecting very small samples from other almanacs—many of them repeating virtually the same lists—and maintaining that the "Mars effect" is present.

II

Suppose, for example, we adopted the same procedure and applied it to Gauquelin's latest European study. The results are interesting: His list of 432 "outstanding" European athletes contains the names of 23 Olympic individual gold medalists, of whom 3 were born with Mars in key sectors, or 13.0 percent.[1] There

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[1] *Olympic Champions—Individual Gold Medal Holders:* J. Barthel, L. Beccali, L. Berutti, C. Besson, F. Blankers-Koen, A. Consolini, Z. Colo, G. Drut, A. Geesink, P. Gross (Mars), A, Hary, K. Hein, W. Holdorf (Mars), H. Kuiper, S. Maury, D. Morelon, M. Ostermeyer, D. Rebillard, P. Trentin, H. Winkler, K. Wolfermann, P. Vianelli (Mars), J. Vuarnet. The data given here and in the following notes were derived from Bernard LeRoy's *Dictionnaire encyclopedique des Sports* (1973) and L. Killanin and J. Rodda, *The Olympic Games* (1976).

are also 8 gold medal Olympic team players, with Mars only appearing once.[2] This total of 31 individual and team gold medalists has Mars present 4 times, or 12.9 percent. This is close to the figure of 13.5 percent for the total U.S. sample; but it is surely surprisingly low if Gauquelin's theory is correct, since by his criteria Olympic gold medalists are the crème de la crème of sports champions. Gauquelin has emphasized that in the U.S. study 7 of the 20 Olympic champions (the correct figures are 8 and 26) were born with Mars as a key sector. He does not mention, much less explain, this difference.

However, in his new European sample, Gauquelin includes not only Olympic gold medal winners but also silver and bronze medal holders. Interestingly, he did not include silver and bronze medal holders as "outstanding" in his post hoc analysis of the U.S study, although there were 11 of them—with only one born with Mars in a key sector (9.1 percent). In his new sample, he includes 22 individual silver and bronze Olympic medalists, with Mars appearing 3 times.[3] There are also 3 silver and bronze medalists who won the medal as members of teams, and Mars appeared twice.[4] The total silver and bronze medalists is 25, with 5 of them, or 20.0 percent, born with Mars in a key sector. However, when the 31 "outstanding" gold medalists are added to the 25 silver and bronze medal holders in Gauquelin's new sample, for a total of 56, Mars appears in a key sector 9 times, or only 16.1 percent.

One can play the numbers game in many ways. Gauquelin, for example, makes reference to the list of "Notable Sports Personalities" published in the *World Almanac.* This list contains only 7 European sports champions (where the countries are indicated), but only 2 appear in Gauquelin's studies (Fanny Blankers-Koen and Jean Claude Killy), both Olympic gold medalists, neither of whom show the "Mars effect." One may ask the same question Gauquelin asks us: Do his own samples contain internationally famous sports stars? For example, *The Olympic Games,* edited by Lord Killanin (current president of the Olympic International Committee) and John Rodda (Macmillan, 1976), contains a list of "Who's Who in the Olympic Games." This is a list of notable individual gold medal champions who are supposed to have achieved "some higher distinction." Most have won more than one medal. In this list there are 13 names that appear in either Gauquelin's original European study of 2,088 or in his new European study, and 7 that appear in the U.S. study, for a total of 20 names.[5] Only two of these champions display the

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[2]*Olympic Champions—Team Gold Medal Holders:* F. Den Hartog, E. Dolman, M. Lauer, H. C. Magnan, E. Monti (Mars), D. Revenu, W. Zimmerer, G. Zoetemelk.

[3]*Olympic Champions—Individual Silver Medal Holders:* C. Arabo, H. Baumgartner, M. Dupureur, D. Debernard, A. Famose (Mars), S. Goitschel (Mars), G. Hennige, L. Lacroix, S. Lopopolo, I. Mir, N. Napoth, E. Puttemans, S. Reding, C. Schiprowski. *Bronze Medal Holders:* M. Gommers, G. Lammers (Mars), S. Morale, A. Moscone, E. Ottoz, P. Pera, R. Schiemeyer, F. Steurer.

[4]*Team Silver Medal Holders:* M. Germar, M. Rozier (Mars). *Team Bronze Medal Holders:* A. Mouyal (Mars).

[5]"Who's Who in the Olympic Games" (champions who have achieved "some higher distinction"): *From 2nd Gauquelin European study:* F. Blankers-Koen, D. Morelon, M. Ostermeyer, P. Trentin, H. G. Winkler, A. Geesink. *From 1st Gauquelin European study:* L. Faggin, L. Gaudin, M. Goitschel, A. Jousseaume, J.-C. Killy, D. d'Oriola, P. Jonqueres d'Oriola. *From the U.S. study:* G. A. Davis, R. Button, H. Dillard (Mars), S. Lee, D. Schollander (Mars), M. Spitz, W. Steinkraus.

"Mars effect" for a total of 10.0 percent. None of the 13 European Olympic champions listed display the "Mars effect"! How is this possible if the Mars effect applies to the "most outstanding"?

III

In addition to the objections given above, we also take exception to Gauquelin's claim that his subsample of 88 from the U.S. study precisely displays the "Mars effect." For at the time of birth of these "outstanding" sportsmen (see Table 12, *SI*, op. cit.), Mars appears in sectors 7 and 8 more often than in key sector 4, and as often in sector 10 as in sector 4. Equally pertinent, even the so-called Belgian Comité Para's sample does not fully replicate Gauquelin's "Mars effect," as he has maintained; for although sector 1 had a higher frequency than expected, sectors 9 and 10 are higher than key sector 4, and 8 is considerably higher than chance (see Table 11, *SI*, op. cit.). Gauquelin has not replied to these criticisms.

Incidentally, Gauquelin is correct in noting typographical errors in the confidence limits given in Tables 4 and 9 (*SI*, op. cit.) in our response to his criticisms of the U.S. study. The confidence limits in Tables 4 and 9 should read $(19.2 \pm 9.0)\%$ and $(16.6 \pm 5.4)\%$, respectively.

IV

Unfortunately, we have not yet been able to complete our analysis of Gauquelin's latest European sample, because we have not yet received all of the data from him. In his new study, Gauquelin tells us that the "Mars effect" appears for a list of 432 names. These names were selected by him—using his own criteria of "fame." He also says that the "Mars effect" is absent in a list of 423 "lesser" athletes—again designated by him. We wrote to Gauquelin, in San Diego, asking him for the list of 423 "lesser" athletes, with their places and dates of birth and their Mars positions. He replied in a letter dated February 13, 1980: "I would be glad to send you the list of 423 names in the sample of the less successful sportsmen in my new European study of sports champions. Unfortunately, the list is in my files in Paris and I cannot send it while I am in San Diego." He did say that if we would send him a photocopy of the French *Dictionary of Sports* he would give us a fairly accurate list of the French names—but without their Mars positions indicated. Gauquelin also states in his letter of February 13, contrary to what he says in his response above, that there were "20/25 cases of unknown Italian cyclists" who were also excluded from his recent study. Most of the champions in this study were derived from the French *Dictionary of Sports*, which contains a total of approximately 3,300 sports champions. Of his sample of 432 "outstanding" athletes, he selected approximately 353 from this reference book. An inspection of the names on his list shows that many seem barely to qualify as "outstanding" (for example, J. de Schoenmaecker, R. Stommelen, W. Teirlinck, R. Van Linden, E. Peelman, R. Rosiers, R. Duquesnoy, and others)—at least when the rigorous criteria similar to that he employs in evaluating the U.S. sample are applied.

We cannot complete our analysis of Gauquelin's study until we have a list of those that Gauquelin excluded from his sample, along with their birth dates and Mars positions, and we shall reserve extended comment until we receive that list. But we reiterate our reservation about his procedure. For, as we have seen, he has

included Olympic bronze and silver medal winners in the list of 432 "outstanding" sportsmen; and he selects all soccer players who were elected at least *once* to the French national team, but basketball, handball, and rugby players only if they were elected *ten or more times*. These differences in criteria have not been explained by Gauquelin.

V

We think that there is a contradictory aspect to Gauquelin's entire work, as we already pointed out. In the original study of 2,088 champions, he did not seem to employ the same restrictive criteria that he now uses. Many athletes selected then would seem to be "lesser" athletes by his present standards—for example, Noel Baudry was in the original study as a rugby player and displays the "Mars effect," but he had been selected only six times for the national team (not ten, as now required). Similarly, Adolph Bousquet displays the "Mars effect," but is listed only 3 times, and Lucien Marracq, also with the "Mars effect," had been listed only once. Rugby player Roger Lerou never made an international team or won any outstanding competitions but displays the "Mars effect."

The selection of French soccer players in the first study is questionable. For example, Simon Zimny was chosen though he played only once in international competition against the Swiss; Georges Rose played once against Luxembourg; and Jacques Leenaert is on the list even though there is no documentation that he played in any international game. All three show the "Mars effect," however. Similarly, whether Clement Duhour or Christian Collardot, listed under Track and Field, who both show the "Mars effect," would now qualify, or whether many of the French boxers would, is highly doubtful. None of the above are "outstanding" or "internationally famous." Many other illustrations could be given.

Even more disturbing is the fact that Gauquelin, in his new study of 432 athletes, has excluded from his sample those champions whom he thinks are not "outstanding" enough, and we have no way to check the list. We note that some famous champions in the *Dictionary of Sports* (such as Max Schmeling, the famous German world champion boxer, and Manuel Orantes, the tennis player from Spain) are missing from Gauquelin's sample of "outstanding" champions, and no mention is made of his attempting to locate them, nor does he mention that the data are unavailable.

Thus, either the original European study is invalid, because it uses looser criteria for the selection of champions, or the new study is invalid, because it is too restrictive in those it includes. He cannot very well have it both ways without being contradictory. If the U.S. study is contaminated with "lesser" athletes, as Gauquelin claims, and is hence invalid, then his original study of 2,088 is suspect because it contains many athletes of similar stature.

We reiterate our earlier point. Why don't the 423 rejected "lesser" athletes, who are at least "famous," display the "Mars effect" at least to some degree? If the "Mars effect" does not apply to famous sports champions but only to *very* famous ones, then how significant is it? Even if Gauquelin's "Mars effect" were confirmed, it would apparently apply, at best, to the relatively small number from each country that make up his list of "outstanding" champions. To show the "Mars effect,"

Gauquelin admits it is not enough to be listed in the *Who's Who* of the field[6]; the athlete also has to satisfy what appear to us to be "subjective" and contradictory criteria laid down by Gauquelin. In the last analysis, the margin of error in differences of interpretation of who is and who is not truly "outstanding" has to be only 5 percent to show results in the direction of the "Mars effect" that Gauquelin desires.

VI

We wish to set the record straight. Nowhere did we wish to suggest that Gauquelin "cheated," and we regret any such implication. Although one of us (Paul Kurtz) spot-checked a number of Gauquelin's original files (of his study of 2,088) during a visit one afternoon to Gauquelin's office in Paris, and was impressed by the "meticulous care" with which the files were maintained, he pointed out that the Committee had not "had an opportunity to verify the registry data first-hand." Thus there has been no direct confirmation by the Committee of Gauquelin's data in his original study of 2,088 champions.

This is one reason we decided to do an independent U.S. study—so that the data could be checked step by step by both Gauquelin and the Committtee. However, in no way did we mean to imply that the Gauquelins had deliberately falsified or misrepresented their data or calculations.

Our remarks in our "Response to the Gauquelins," (*SI*, op. cit., pp. 44-63) were meant to illustrate how treacherous it can be to attempt to draw statistical conclusions from small samples, and especially by post hoc selection of the data. We do not question the Gauquelins' integrity but rather the significance of their alleged results (even tentative ones) obtained by such selection, no matter how well intentioned it may be.

We believe that, if real, the "Mars effect" is an astonishing phenomenon, for it is entirely unexpected on the basis of all we know about physical and biological science. That the effect is unexpected does not, ipso facto, make it invalid, but we insist that extraordinary results require extraordinary evidence. At best, the "Mars effect" is a weak one, and hence would require great statistical care to confirm it. Our own independent study does not confirm the effect, however; and in our judgment the subsequent arguments of the Gauquelins, for the reasons we have given, do not comprise the extraordinary evidence required to be convincing.

We reiterate that it is impossible to prove a negative by statistical arguments, and we make no claim that the Gauquelins' "Mars effect" is positively disproved. On the other hand, we believe that our sample of 408 U.S. sports champions, a large portion of which evidently satisfies the Gauquelins' own strict selection criteria, should have shown some indication of the effect if it were real and general.

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[6]Gauquelin makes this clear when he says: "As we demonstrated, to be listed in a national *Who's Who* does not offer sufficient standard for observing the Mars effect. *In Les Hommes et les Astres* (1960) we showed that 599 footballers (soccer players), who were in the first division of the Italian Football League but who had not achieved international status, did not show the Mars effect either." (*SI*, op. cit., p. 36).

VII

We re-state our conclusions: *An effort at independent replication of the "Mars effect" among 408 U.S. sports champions shows no evidence that such an effect exists, nor does Gauquelin's new study of 432 European sports champions make the case any more convincing.*

Paul Kurtz, Marvin Zelen, and George Abell

Evolution and Creationism

Science is more in the eye of the beholder than the CSICOP might at first realize. The evidence for the paranormal is quite soft, true, and if the SKEPTICAL INQUIRER stuck to this subject, perhaps the notion of what is scientific would not arise. Claims that the currently known laws of science are violated should be backed up with hard, not soft, evidence.

The article "Science and Evolution in the Public Eye" (*SI*, Fall 1979) is particularly authoritarian. The readers should have been informed that evolutionary theory is now in a state of exciting ferment and is undergoing drastic revision. The old view that species evolve into others by way of accumulating small changes is being replaced with one that admits of wholesale, catastrophic change. A French anthropologist, René Thom, has set forth the mathematical background for such happenings in *Structural Stability and Morphogenesis* (1972) and recent reports in *Science* deal with wholesale chromosome reshufflings rather than mutations at specific gene loci. Many biologists are speculating that a large part of the chromosomes consist of DNA that does not contain genetic information. It is this surplus DNA that allows for the reshuffling.

A problem in evolutionary theory, noted by Darwin himself, is that there seem to be more gaps in the fossil record than even the scarcity of fossils would allow. The usual explanation is that transitional forms are unusually short-lived, and the reason for this is that they fit into no ecological niche. Now why the ecological niches should be so nonuniform has not been explained and it is no explanation to assert that the gaps in the record *have* to fit the gaps in the niches. An assertion like this is dogma, not science.

What gives the article in your magazine its authoritarian tone is its assertion that evolutionary theory is established and not itself in the process of evolution. Even more out of keeping with the scientific temper are the ad hominem attacks on theories of separate racial development because they are expounded by the Ku Klux Klan, an organization with "blatantly racist motives." Blatantly racist the Klan is, but the reader should have been informed that the origin of human races is a matter of serious anthropological controversy. Carleton Coon, a retired

anthropology professor formerly at Harvard, argued in *The Origin of Races* (1962) that the record of pre-sapiens bones shows racial characteristics. In each of five races he traces the independent and parallel evolution from Homo erectus to Homo sapiens. Whether one wishes to call racist Coon's conclusion—that the races of man are considerably older than had often been thought—is unimportant. What does matter is whether the work by a distinguished anthropologist has been *successfully* refuted.

If your author is unaware of Coon's work, perhaps she should not be teaching anthropology. If Coon was mistaken, your readers should certainly know where, when, and by whom this was determined. Otherwise, the purpose of the CSICOP is not a scientific but a political one—to label views you don't like as racist, unscientific, or some other epithet.

Frank Forman
Bethesda, Md.

Laurie R. Godfrey replies in the following article

The Misunderstanding of Evolutionary Biology

Orthodoxies are in the eye of the beholder. Last week a colleague and I received a letter concerning our proposed paper examining the relative merits of the neo-Darwinian vs. the Stanley-Gould model of evolutionary change. The letter conveyed the opinions of two prominent physical anthropologists who reviewed our proposal and concluded that the paper should *not* be presented at our profession's forthcoming meeting—presumably because neither of the reviewers took seriously the Stanley-Gould challenges to neo-Darwinism. I was amused when, in the same day's mail, I received Forman's letter to the SKEPTICAL INQUIRER accusing me of defending old orthodoxies and pretending that evolutionary theory is not in a state of "exciting ferment" or "undergoing revision." To the contrary, I consider the modification of old orthodoxies by hypothesis-testing critical to the advancement of scientific thought.

The problem I have in responding to Forman's letter is that it does not address the issues I raised in my SKEPTICAL INQUIRER essay. I did not present hard evidence for evolution or discuss pros and cons of various theories because this is not the SKEPTICAL INQUIRER's role. Furthermore, Forman seems to misunderstand my main point (which can hardly be considered authoritarian): that hypothesis-testing and problem-solving should be built into introductory courses and popular science literature dealing with evolutionary biology. Of course, specialists reading such journals as *Evolution, American Naturalist, Paleobiology, Nature, Journal of Human Evolution* are not so naive, but most nonspecialists are unaware that evolutionary biology *explains* anything. They understand neither the *evidence* nor the *data* that are being explained. With evolution widely perceived as an elaborate "just-so story," it is not surprising that many people are unable to evaluate "scientific creationism." They are at the mercy of trusted "authorities."

My second point—that scientific creationism is poor science—may sound dogmatic or authoritarian; but, if one accepts my premise that science is a set of

disciplines that has built-in mechanisms—in rational hypothesis testing— for the destruction of its own orthodoxies, it is impossible to conclude that scientific creationism is good science.

Biological evolutionism is based on the premise of *genetic continuity*, which is itself a testable hypothesis.[1] Given genetic continuity, numerous evolutionary explanations, Darwinian and non-Darwinian, are possible; and, indeed, evolutionary biologists have been challenging orthodoxies, debating, and revising explanations for well over a century. Most scientists, I dare say, are willing to see even their pet hypotheses crushed given an alternative with greater powers of explanation and prediction (witness the rapid acceptance of the previously unpopular continental drift hypothesis when the predictive success of plate tectonics became clear).

Creationism is based on the premise of *genetic discontinuity*. Given genetic discontinuity, numerous creation-based explanations *should* be possible (indeed, consider the global diversity of creation myths); yet the scientific creationists begin with a specific and detailed explanation of the universe and require the *acceptance by belief in the truth of this "theory"* before one can even gain membership in their various research organizations:

> All members . . . must subscribe to the following: . . . The Bible is the written Word of God, and because we believe it to be inspired thruout, all of its assertions are historically and *scientifically true* in all of the original autographs. [Emphasis added.] To the student of nature, this means that the account of origins in Genesis is a factual presentation of simple historical truths . . . All basic types of living things, including man, were made by direct creative acts of God during Creation Week . . . the Noachian Deluge, was an historical event, worldwide in its extent and effect. [Creation Research Society, Ann Arbor, Michigan. Membership application form.]

> The Bible is the written Word of God. It is inspired and inerrant throughout. Its assertions are historically and scientifically true in all the original autographs . . . [Creation Social Science and Humanities Society, Wichita, Kansas. Membership application form.]

This dogmatism is what makes scientific creationism anathema to most scientists, and, indeed, to most theologians.

Furthermore, scientific creationists distort the evidence to fit their theory to such an extent that it seems difficult to take their claim of scientific objectivity and impartiality seriously. How, for instance, can one interpret the following "explanation" of biostratigraphy as anything but satire?

> It is true that in a given area where several periods are represented, the deepest layers generally contain less complex forms than the shallower layers. This could be explained by the fact that most fossil material was laid down by the flood in Noah's time. As the flood waters rose, less complex forms, being less able to escape, would be buried first. More complex and more mobile forms could move to higher ground. [Moore and Slusher, eds. 1970, p. 405. (This book was prepared for educational use by the Textbook Committee of the Creation Research Society.)]

One of the current issues in evolutionary biology is the debate between "phyletic gradualists" and "neo-catastrophists." Evolutionary biologist C. H.

Waddington (1975) made René Thom's (1975) mathematical "catastrophe theory" the central theme of one of his essays, thereby helping to revive evolutionary "catastrophism." Several years earlier paleontologists Stephen J. Gould and Niles Eldredge introduced their theory of "punctuated equilibria" (Eldredge and Gould 1972) based *in part* on the research of neo-Darwinian traditionalists Ernst Mayr and George Simpson. Løvtrup (1974), Frazzetta (1975), Gould (1977), and others reviewed evidence that slight changes in development can have major ("saltational") effects on adult phenotypes and suggested that such changes may explain rapid evolutionary change. Research on epigenetics (Løvtrup 1974), chromosomal reshuffling and regulator gene processes (King and Wilson 1975; Wilson, Carlson, and White 1977), clines (Endler 1977), heterochrony (Gould 1977), biostratigraphy (Hallam 1977; Stanley 1979) all bears on this controversy. Unfortunately, the ideas of neo-catastrophists have been misinterpreted and misused by creationists. In this regard, I should point out that saltational evolution is *not* a rejection of the basic evolutionary premise of genetic continuity; nor do Stanley (1975, 1979) and Gould (1980), who make strong statements rejecting the traditional neo-Darwinian explanation of macroevolutionary trends ("ortho-selection"), reject Darwinian natural selection as an important agent of evolutionary change. They merely reinterpret its role.[2]

Since Gould was one of my teachers, it was with some hesitation that I selected the term "catastrophism" to decribe the miracle explanations of scientific creationists. The saltational evolution of Gould and others and the miracle-based catastrophism of the scientific creationists are not comparable.

Forman alludes to my discussion of racism. I did not intend to equate racism with antievolutionism. Many fundamentalists correctly point out that *evolution* has been abused by racists. However, miracle-origin scenarios often have racist *implications* and the fact is that racists often rely upon miracle-origin myths and/or degenerationism to "substantiate" their arguments. While scientific creationists tend not to be racists, they *do* assert their cultural and moral superiority to "degenerate forms" of belief (Morris 1974, p. 38); they reject cultural relativism and humanism for supposed "scientific" reasons (cf. Morris 1974, pp. 30-39). I would hate to see this viewpoint promoted as good biology in our schools.

Whatever Coon's intentions, *The Origin of Races* (1962) also has racist implications (Lieberman 1975). But Forman is incorrect in asserting that anthropologists have never successfully debated or refuted it. Coon's book has been criticized from the vantage point of its fossil "evidence" and, especially, its shaky genetic model. By the time I was a graduate student at Harvard this book was considered passé. *The Origin of Races* was a hot issue in the early 1960s—widely discussed in journals such as *Current Anthropology* and *American Anthropologist*. Materials easily available to the public that review Coon's work include Montagu (1964) and Alland (1971).

Evolutionary biology is a complex set of theories that attempt to explain similarities and differences among organisms in space and time. It is a science and should be *taught* as science—not dogma. No science is static. Nowhere in my essay did I assert that "evolutionary theory is established and not itself in the process of evolution." If this were true, it would not *be* science. But refutation based on misunderstanding is not refutation, and my experience has shown me that evolutionary theory is widely misunderstood by people who claim to understand it. This misunderstanding and its relationship to the growth of scientific creationism

in the increasingly conservative political climate of the late 1970s was the subject of "Science and Evolution in the Public Eye."

Laurie B. Godfrey
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Five recommended paperbacks on evolutionary theory written for popular consumption:

Gould, S. J. 1977. *Ever Since Darwin.* New York: Norton. (See also "This View of Life" — Gould's monthly column in *Natural History.)*

Savage, J. M. 1977. *Evolution,* 3rd ed. New York: Holt, Rinehart and Winston.

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NOTES

1. Evidence for genetic continuity among organisms comes from the fields of biogeography, comparative ethology, comparative anatomy and embryology, molecular biology, genetics, and paleontology. Also, contrary to popular belief, the process of speciation (the origin and multiplication of species) has been observed (cf. Stanley 1979), and the genetic continuity of species (however radical the morphological change) is clear. Speciation is one focal interest of modern evolutionary biologists not discussed by Charles Darwin in his *Origin of Species*.

2. I recommend that laymen interested in these developments read Frazzetta (1975) and Stanley (1979) before delving into more difficult books and journal articles. For an up-to-date explanation of neo-Darwinism, Dobzhansky et al. (1977) is a very good text. Futuyma (1979) presents gradualist and opposing viewpoints in an even-handed way. Another up-to-date text on evolutionary theory is Ayala and Valentine's (1979) *Evolving*.

The following two responses continue the exchange between Jule Eisenbud and James Randi that began in the Spring 1980 SKEPTICAL INQUIRER.

Cottrell Tests: Jule Eisenbud Replies to Randi

I ought to say something about Randi's seeming bafflement as to why we did not include in our testing procedures the "show-biz" type of routines he and his co-testers used, a "direct set of understandable and easily followed procedures" amenable to "careful" (sort of) observation. He professes to be unable to see the value of our having introduced a "complex and puzzling" variety of conditions with all sorts of scientific frills and furbelows.

Why this should be so difficult to grasp escapes me; but since there is little doubt that these procedures would be just as effective against experts as they were in Cottrell's case, perhaps a direct experience of them by Randi would do what plain English seems unable to. If, therefore, Randi can match Cottrell's results under the same conditions in force (easily seen in more than 40 hours of tapes), I will gladly stake him to his round-trip fare, hotel, and meals for as many days as it

might take him to do a reasonably reliable control series. (At the same time, so long as he has gratuitously dragged Ted Serios into the discussion—as if this somehow self-evidently discredits anything I have to say—I will be happy to extend his visit and to provide the same facilities as were used in testing Ted, same crew and all.) Randi may recall a challenge issued on this score in 1967 (J. Eisenbud, "The Cruel, Cruel World of Ted Serios," *Popular Photography*, November 1967; also the "Today Show," October 4, 1967), a challenge that still has not been picked up by him or anyone else. If Randi doesn't find this offer quite the one he can't refuse, I might go a little higher—say even a bonus like a three-day, all-expense trip to Grossinger's in the Catskills.

So far as Cottrell goes, a direct control of this kind would obviate any questions about the procedures from which Randi's conclusions were allegedly drawn. If his inferences are as reliable as he thinks they are, this would only underscore the fact. I must confess to not being overly impressed by his declaration that "we really did keep careful and complete records," and his belated show of "figures" (a good share of which I defy anyone not only "to find deeper meaning in" but even to make any sense of at all in terms of the information yet provided; in fact I suggest Randi consult *Probability and Statistics in Psychological Research and Theory* by Donald W. Stilson, Ph.D. (Holden-Day, 1966), before any further obfuscation is proffered. As far as I am concerned, at any rate, until Randi can meet this test, he has not discredited Cottrell, merely bad-mouthed her.

Jule Eisenbud

James Randi Responds

Jule Eisenbud simply cannot grasp the one most important fact about scientific procedure applied to parapsychological research: the question is not "Can Randi do it?" but rather "Can the psychic do it?" Years ago, Eisenbud challenged me to perform an experiment (to use the term loosely) in which I was to be dressed in a skin-diving outfit, to be drunk, to have my body orifices probed, to be locked away from the camera, and then to produce "psychic" photos, as his now-faded miracle-worker Ted Serios had once done to Eisenbud's exceeding bafflement. He claimed that Serios had been subjected to these bizarre conditions, but he failed to note that Serios had failed to yield a "psychic"photo while dressed up as an angry frog on a toot in a steel aquarium!

It is obvious that one cannot now reconstruct the carnival atmosphere that prevailed at Eisenbud's Serios "experiments" any more than one can re-create the scene of his Cottrell tests, which we have no reason to believe were any more controlled. For this reason alone, I have no interest in assisting at comedy productions that take on the airs of scientific research.

Susie Cottrell was tested in Buffalo under the careful supervision of several scientists, including Paul Kurtz, Jim Pomerantz, and Irving Biederman, with Martin Gardner and myself sitting in to advise on procedure. We set up a situation in which she agreed not to cheat—in writing—and yet, when given the open opportunity, she employed simple sleight of hand, which was recorded on videotape. These tape records were submitted to Frank Garcia, an expert in card-manipulation techniques, who confirmed what Gardner and I had observed, that

Cottrell was employing the same procedure she had used on the Carson show and the same routine that had been described to us as being used at another demonstration of her tricks in another city. When we merely cut the deck—a procedure established as a wise and prudent move in card games—she was unable to perform her trick.

When Eisenbud failed to be "overly impressed" by our "belated show" of figures on the Cottrell tests, he suggests that we did not previously have these figures and facts to present. He is wrong. We have now, and had then, a complete record of the event in great detail, impossible to present in a SKEPTICAL INQUIRER report without running on for extra pages. Recourse to statistical tomes is not necessary in this case; the smoking gun is quite evident. We are not accustomed to resorting to rationalizations to excuse the subject's failure. Eisenbud must realize that we seek not to support an unproved premise, but to determine the truth.

"As far as I am concerned . . . Randi . . . has not discredited Cottrell." I am relieved to note that Jule Eisenbud speaks only for himself.

James Randi

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From Our Readers

Dowsing tests deficient?

As a recent subscriber to your publication I must say that it is quite unlike any other magazine I have seen and is enlightening in many respects. However, the article by James Randi on dowsing in the Fall 1979 issue seems to sidestep many of the requirements of a controlled scientific experiment. In fact the experiment seems to be as biased toward disproving dowsing as those in the "popular press" are biased toward "proving" such paranormal abilities.

Although the dowsing experiment was performed under controlled conditions—that is, the site selection, preparation, etc., were chosen by the experimenter rather than by the subjects—it was definitely not a controlled experiment. A controlled scientific experiment would have a *control group* of subjects, and conclusions would be based on the *difference* in performance of the avowed dowsers and the control group of "ordinary" people. As any high school science student knows, the purpose of the control group is to cancel out the effect of outside influences on the experiment and thus isolate the effect of the parameter being studied. Experimenters often go to great lengths to ensure the equality of test and control groups, using such techniques as double-blind studies, where even the experimenter does not know which group is which.

Another objection is the poor design (by modern scientific standards) of the dowsing experiment. One shortcoming is that the pipe layouts were determined by one human being and the test subject was another human being. This actually favors successful dowsing because human beings are remarkably similar in their conception of randomness. Ask any two people to mentally conceive a sequence of random digits and they will be not only statistically similar but decidedly nonrandom. The experiment would have been much better if flexible hose had been used and a computer had randomly selected the paths (within certain constraints, such as a constant hose-length). An added benefit would be the ability to compare dowsing results statistically with chance results. Also the numerous excuses made in the article for lack of time, lack of water, last minute changes in the plot size, change in procedure based on preliminary results, etc., did little to build confidence that the experiment was well designed.

The most serious objection however is the lack of *sensitivity* in the experiment and the utter impossibility that even an incredibly accurate dowser could have won the prize. To prove this point, let's assume that the pipes were made of iron and that instead of a dowser we have a man with a metal detector. Given the conditions of the experiment (8-cm-diameter pipe buried 50 cm underground with a tolerance band 20 cm wide) it is unlikely that this man could have won the prize either. My trigonometry says that there would

be only a 2 percent difference in signal strength (the pipe is a *line* source, so the inverse square law does not apply) between the center of the pipe path and the edge of the tolerance band, assuming the detector's head was very small. The difference would be even less with typical head diameters of 30 cm. If the detector's signal pickup determined the loudness of a tone, that 2 percent would be only 0.17 dB, considerably below the 1 dB differential threshold for humans. If one hypothesized a "psychic signal" from water flowing in the pipe, not only would the dowser have to be sensitive to differences less than .17 dB, but the psychic signal-to-noise ratio would have to be greater than 34 dB just to get 50 percent of the pegs within limits!

In conclusion, the only point that the dowsing experiment proved is that dowsers grossly overestimate their capabilities. One cannot say that significant dowsing was not demonstrated, only that the sensational type of dowsing seen in old western movies was not demonstrated. Further conclusions will require performing properly designed experiments of greater sensitivity.

Lest one conclude that I am a firm believer in dowsing, I wish to make a couple of additional points. ESP, if it exists at all, is going to be a very small effect when observed under controlled conditions. If it was a large effect, it would have already been accepted as a common everyday event rather than being immersed in controversy. Being a small effect, it will take sensitive experiments to observe it. To give an idea of what a sensitive experiment is, consider the thousands of *tons* of water and other liquids used in neutrino and gravity-wave detection experiments or the great effort being expended to determine if saccharin is really a carcinogen. To me, if ESP experiments can consistently show a statistically irrefutable performance level above chance, even if it is 50.001 percent, an explanation is required as surely as if the performance level was 100 percent. Such an explanation might very well hypothesize "second order" statistical phenomena, much like the quantum mechanical and relativistic effects in physics, which are extremely small under normal circumstance, rather than ESP.

Hal Chamberlin
Manchester, N.H.

James Randi responds:

Mr. Chamberlin makes a number of interesting points, some of which resulted from my necessarily incomplete account of the total protocol employed. All of this is a matter of record but was condensed for SKEPTICAL INQUIRER *use.*

"The experiment seems to be . . . biased toward disproving dowsing . . ." Really? The dowsers had by far the advantage, providing that a power exists to do what they claim. They at all times said that they were confident of success, that they were proceeding successfully, and, afterwards, that they had succeeded. They offered no complaints about protocol, approved each and every step of the adopted procedure, and in fact agreed that no "control" nondowsers were required because it would be impossible to tell if these controls were also using dowsing powers. That all persons possess this power, they agreed. Thus, controls were useless.

"[In] a controlled scientific experiment . . . conclusions would be based on the difference *in performance of the avowed dowsers and the control group of 'ordinary' people." Not so. There was no ambiguity at all about the rules, and careful and agreed parameters were established in advance, well beyond those suggested by the dowsers, in order to give them an advantage. The "difference" would only be a criterion if the rules allowed any uncertainty in judging. If an archer says he can hit a bull's-eye 7 times out of 10, it is not at all difficult to decide whether*

he makes good his claim. It is a matter of simple numbers. We counted the percentage of pegs that were placed by the dowser within certain limits of the buried pipes. It was a matter of mathematics, a digital measurement rather than an analog.

Chamberlin complains of the "poor design" of the tests. He says the layouts were "determined by one human being." True, they were, but in a random manner. A heavy ball was rolled from outside a delineated square on a large board and transferred to the master chart. It was a practical and efficient method. And it worked. The eventual pipe layout was "rounded off" to accommodate the materials used, of course; and hairpin turns had not been allowed by the dowsers, so these were cut out for that reason.

Flexible hose, preferred by Chamberlin, was not usable because one of the dowsers had said that rubbery substances inhibited his powers. He had tried a flexible material once, and failed. We were required to go along with that requirement. And as for objections concerning our excuses re lack of time and water, and change in plot size, these were unavoidable. The test, as it was, cost many thousands of dollars, and, most important, all the subjects agreed to the changes without reservation. An exception was the depletion of the water, which served to prove that Professor Borga continued to imagine he was dowsing long after the water had ceased to flow.

Next, Chamberlin raised serious objection to the "lack of sensitivity*" in the tests. The fact is that all subjects told us what they could do, and within what limits. We then* increased *the diameter of the pipe, raised the depth to* half *what they declared suitable, provided* twice *the volume of flow, and gave them* one-third more *leeway in approaching the actual pipe path in their determination of its position. The actual conditions thus gave them* many *times the optimum conditions and parameters, and all agreed we had been most generous. One declared that I was a fool to be so liberal, since it was obvious (to him) that he had won the prize.*

Remember, the dowsers were able to detect the pipe very positively *in an exposed condition and also as it went beneath the ground to the junction point, at which position it was 50 cm deep. Only when they did not* know *the location did they fail, though they still got strong reactions.*

Furthermore, none of the dowsers agreed on where the natural "underground streams" were in that designated area. And when a strong (very strong) magnet was buried just 3 cm in the ground during a subsequent test (unreported), neither of the two dowsers who said they were very good at that test could find it in a one-meter-square area. However, when the location was given, the sticks twisted and dipped appropriately.

In conclusion, Chamberlin says that, if dowsing powers exist at all, they are doubtless very small in order. Agreed. But according to the experts in this subject, dowsers can even "unfailingly" detect small currents flowing in a copper wire. Surely that is an easy test, and simple to conduct? Yet the president of the American Society of Dowsers, through his spokesman, Z. V. Harvalik, says that tests of dowsers are "superfluous" and that dowsing is "amply established." I invited this convinced dowser to win my $10,000 prize in tests that he would consider adequate. I await a response.

Tests of "thoughtography"

Scott and Hutchinson committed several errors in their article in your Spring 1979 issue concerning tests of Masuaki Kiyota's "thoughtography." Included among them is Hasted's hailing the production of a "whitey," only for the sales representative of Polaroid to point out a hole in the camera. Scott and Hutchinson, who were not present, are not to know the several gross inaccuracies in that statement, which comes close to being

legally actionable.

Morris Smith is supposed to have told Scott and Hutchinson that he wasn't exactly sure how a film pack can be taken to pieces, each frame being exposed to light, and reassembled. But, in fact, he explained this in detail to Hasted, with a description of the traces of evidence that would remain and the statement that he had searched the pack for such traces and not found them. (Hasted has since searched other, more recent Masuaki packs and found nothing.) This differs from Scott and Hutchinson's statement that Morris Smith twice said it was not possible to tell if the film pack had been tampered with. Hutchinson has more recently claimed to have found a method without leaving traces, but Polaroid has not investigated this claim to my knowledge.

Finally, journalist David Tharpe's claim that the camera and the film had not left his or his girlfriend's sight during the two-hour lunch-break should not be dismissed. No doubt he will communicate, Sir, with you.

John B. Hasted
Birkbeck College
University of London
London, England

Michael J. Hutchinson replies:

Although the article in the SKEPTICAL INQUIRER *was a joint effort between Christopher Scott and me, I alone was responsible for gathering the information concerning events that took place before our arrival at Granada's tests. It is therefore only right that I alone answer Professor Hasted's letter, which I do with the full knowledge and approval of Christopher Scott.*

I find it difficult to respond to the professor's first paragraph for its lack of information. Just what "errors" is the professor referring to? His "hailing" the production of the whitey, or reference to a hole in the camera? In an attempt to find out I tried to contact Jeremy Fox, the TV producer, and Morris Smith, the technical (not "sales") representative from Polaroid, to obtain their comments on the "whitey" incident. Unfortunately, due to a labor dispute that affected Granada Television for nearly three months, it was not possible to contact Mr. Fox. I have subsequently learned that he has now left Granada and is working in the United States.

However, Morris Smith has written to me as follows: " . . . Your report on the 'whitey' incident is exactly as I remember it, with one slight *[my emphasis] difference. It was not a hole that had been left, but the base of the diode was not completely opaque and when the connection was removed light filtered in." This is a more technical explanation than the one given to me by Jeremy Fox. The main point is the same.* It was the removal of the connection that allowed light to filter in.

Perhaps the professor is objecting to the use of the word "hailed," which seems to be the only other possible error. (Unless Hasted had not found the day to be as monotonous as suggested by us.) Both Jeremy Fox, immediately after the event, and Morris Smith, some thirteen months later, have spoken of Hasted's "excitement" upon production of the "whitey." In order not to cause him undue embarrassment, we chose to use the word "hailed." The professor's poor attempt at pointing out "gross inaccuracies" that are "close to being legally actionable" has only highlighted the events of the day.

Morris Smith's method for exposing each frame of the film involves the removal of a staple that holds all eight frames in the pack and prevents them from being pulled out together. Therefore, examination for tampering means looking to see if the staple has been opened. (The staple is fairly wide and is very firmly fixed. Do not imagine that it is like the staples which hold the pages of this journal.) Whilst not impossible, it would be

difficult and time-consuming to remove and replace the staple in darkness.

In looking at Masuaki's film packs, Hasted must be assuming that the only method of faking the exposures is to remove the staple. The method devised by me and described in the SI *article does not require the removal of the staple. (There may also be other ways. Perhaps some imaginative readers have already experimented. If so, we would like to hear from them.) Professor Hasted is urged to try the pinhole method for himself. I will be willing to assist him if necessary.*

In his references to the examination of recent Masuaki film packs by himself, and of my packs by Polaroid, Hasted is getting his priorities wrong. He is satisfied that he is qualified to judge in a matter involving extraordinary claims, but seems to think that Polaroid must investigate my clearly explained method for duplicating these claims. Surely, Professor Hasted, the question is "Can Masuaki Kiyota prove his claim to transfer mental images onto unexposed film?" From the evidence so far, the answer must be "No."

Professor Hasted might be prepared to let children take pieces of metal home to see if they can bend them paranormally, but he shouldn't expect us to believe that they really do so. (One of his "successes" was with a piece of metal that was "paranormally" bent to read "This is fun.") Similarly, he shouldn't expect us to believe that some of Masuaki's photographs were paranormal just because David Tharpe says that the camera and film had not left his sight. This is no reflection on Tharpe's honesty. The lunch-break was two hours long, and anything could *have happened in that time. It's not that this was bad protocol, Professor Hasted. It was no protocol at all.*

The U.K. psi scene

I am happy to report that the library of this university now takes your journal. The first number to appear, in November 1979, was the Spring 1979 issue, so I must suppose that its journey across the Atlantic was somewhat delayed, doubtless by malign influences emanating from the Bermuda Triangle.

The editorial of that issue struck a moderately confident note, claiming some setbacks for the forces of unreason. I only wish that I could concur as a result of my own observations over here. Alas, I cannot. Great publicity was given to the award by the University of Cambridge of a Ph.D. for ESP work claiming positive results; and the *Guardian* newspaper gave it the full treatment we have come only too readily to expect. The usually admirable journal *Encounter* promises an article on progress in parapsychology, to be written by Dr. Beloff, whose convictions are known. Only last week I spoke to a child-psychiatrist who told me that it no longer caused surprise to hear perfectly ordinary parents of disturbed offspring asking whether the unfortunate child might be "possessed."

O tempore! O mores!

M. Hammerton, Head
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